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Lotharingia as a center of Arabic and scientific influence in the eleventh century

Western European astronomers before the end of the tenth century were greatly handicapped in their work because of the lack of proper instruments for making observations. Up to that time their principal instruments were the Greek armillary sphere and the sun-dial. In reckoning the dates of Easter and the other movable feasts, and the lengths of years, months and days, they depended upon Easter cycles or tables which were founded upon those made at an earlier time by the Alexandrians. Although for quite some time before the tenth century they had realized that their calculations were not correct, they were unable to improve upon them because they had not yet received from the Arabs better astronomical theories and instruments.

Towards the end of the tenth century knowledge of the Arabic astrolabe began to penetrate into the Latin West. From the manuscripts which have so far been discovered, this astrolabe must have been used first by Lotharingian geometers and astronomers. Professor J. W. THOMPSON in his article, « The introduction of Arabic science into Lorraine in the tenth century (1), » has already discussed the question of Arabic science in the Latin West. His conclusion may best be summed up by the following quotations; « Arabic science was introduced into the schools of Lorraine and was cultivated there before GERBERT (2) » and later, « I am convinced that the schools of Lorraine in the last half of the tenth century were the seed-plot in which the seeds of Arabic science first germinated in Latin Europe, from which the

(1) *Isis*, 12, 184-194, 1929.
(2) *Ibid.*, 184.

knowledge radiated to other parts of Germany—witness HERMANN CONTRACTUS in Reichenau—to France, and especially, owing to the preference of KNUT the Great for Lotharingian churchmen, into England. (3) »

It is not strange that the schools of Lotharingia should have

SKETCH MAP OF LOTHARINGIA, 950-1050.

been the earliest centers in Western Europe north of the Pyrenees in which Arabic learning was fostered, because in comparison with the famous schools of the tenth century, such as Reichenau, St. Gall, Reims, Paris, Chartres, and Cluny, those of Liége were especially outstanding for their real love of scientific subjects.

(3) *Ibid.*, 191.

Although this particular interest became evident first in Liége, it was also present in Gorze (4), Cologne, Prüm, Lobbes and other Lotharingian cities. Throughout the latter part of the tenth century and during the eleventh, there was a lively exchange of letters and manuscripts on scientific subjects among the scholars of these cities, and from the latter to other places just outside Lotharingia, such as Reichenau.

The interest in astronomy at Liége started in the time of EURACLUS (5), a Saxon, who was bishop from 959 to 971. According to REINERUS (c. 1190), EURACLUS had studied first at Cologne under archbishop BRUNO, brother of OTTO I, and then under RATHERIUS, the famous bishop of Verona. When he became bishop he immediately began to improve still further the schools in his diocese (6). Due to his influence, interest in scientific subjects increased rapidly. That he himself was no mean astronomer is evident from the story of his expedition to Italy with the troops of OTTO I in 968. On December 22 there was a solar eclipse and EURACLUS was able to calm his frightened troops by assuring them that this was only a natural phenomenon, and that the sun would soon reappear. REINERUS says that he got his astronomical ideas from « PLINY, MACROBIUS, and CHALCIDIUS, and many others, astrologers as well as compotists (7). »

At the death of EURACLUS, a better known scholar, NOTGER (8), became bishop (971-1008) and under his direction Liége became one of the most important intellectual centers of Europe. NOTGER had been educated at St. Gall where astronomy and other sciences were of great importance, and from his youth he had given promise of becoming a great scholar. He has been called « the second

(4) For the Gorzean Reform and the intellectual activities there at the time of JOHN OF GORZE (c. 960), see J. W. THOMPSON, *op. cit.*, 188-189.

(5) REINERUS, Monachus S. Laurentii Leodiensis, *Vita Euracli*, I, in B. Pez, *Thes. anec. noviss.*, IV, III, 155-166, 1721-1729.

(6) ANSELM, *Lectiones majusculis tradere*, 24, gives a delightful picture of EURACLUS as a teacher.

(7) REINERUS, *op. cit.*, chapter 24.

(8) G. KURTH, *Notger de Liége et la civilization au X^e siècle*, 2 vols., Paris, 1905. H. PIRENNE, *Histoire de Belgique*, 1^3, 157, Brussels, 1909. M. MANITIUS, *Geschichte der lateinischen Literatur des Mittelalters*, II, 219-228, Münich, 1911-1923.

founder of Liége (9), » because he built so many churches, monasteries, fortifications, hospitals, and imported various kinds of artistic objects from many countries. But above all he devoted great attention to the schools of Liége and encouraged even more vigorously than his predecessor, the study of mathematics and other scientific subjects (10).

While NOTGER was bishop, there were several astronomers at Liége, for instance ENGLEBERT of St. Lawrence, a compotist, who investigated the number and movements of the stars, the sun and moon, as well as the causes of earthquakes and tides (11). An interesting contemporary of ENGLEBERT was RADOLF (12), from whose correspondence with his friend RAGIMBOLD OF COLOGNE we get several clues concerning the state of mathematics in Lotharingia at the beginning of the eleventh century.

Very little is known about the life of RADOLF except that before he became one of the teachers in the cathedral school at Liége he had studied with FULBERT at Chartres. That he too was interested in mathematical subjects may be learned from his correspondence (c. 1025) with his friend RAGIMBOLD, « magister » of the schools in Cologne. These letters deal chiefly with questions of geometry and arithmetic but Letter V does contain one very precious bit of information concerning astronomy. In this letter RADOLF says that he has an astrolabe and continues, « I would willingly send it to you in order that you might examine it, but it is my model. If you wish to know what it is, come to the festival of St. Lambert, you will not be sorry for it. It would be useless for you just to *see* an astrolabe. (13) » Unfortunately RADOLF does not give a description of the instrument nor does he tell us where he got it; however from other manuscripts found in this region it is certain that Lotharingian astronomers were by this time using an Arabic type of astrolabe. Also there is

(9) G. KURTH, *op. cit.*, I, 130-169.

(10) *Ibid.*, I, 251-299.

(11) REINERUS, *De claris scriptoribus*, book I, chapter 9, in B. PEZ, *Thes. anec. noviss.*, IV, III, 20-52.

(12) P. TANNERY and Abbé CLERVAL, « Une correspondance d'écolâtres du XI^e siècle, » in *Notices et extraits des manuscrits*, XXXVI, 487-543, 1899-1901. M. MANITIUS, *op. cit.*, II, 778-781.

(13) RADOLF would undoubtedly not have been so eager for RAGIMBOLD to see his astrolabe if it had been of the ordinary Greek type.

no evidence from the writings of either GERBERT (14), or FULBERT OF CHARTRES (15), or ABBO OF FLEURY (16), all outstanding teachers of that period, that they were using Arabic astrolabes in their schools. So that RADOLF must have found his model in Lotharingia.

In the second part of the so-called *Geometria Gerberti* (17), designated by TANNERY and CLERVAL as the *Geometricales diversitate* (18) there are several chapters containing descriptions of a quadrant astrolabe, a type developed by the Arabs and used for surveying. According to TANNERY and CLERVAL this second part of the *Geometria* was assembled after the time of GERBERT. They also say that Ms. 7377c in the Bibliothèque Nationale is the oldest copy yet discovered which contains this collection, and most important to note, the article on the astrolabe is in that group of treatises written in Lotharingia in the eleventh century (19). Although RADOLF does not tell us what kind of astrolabe he had, it might well have been this same type as his principal interest was in geometry and this instrument described in the *Geometria* was used by geometers and surveyors.

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(14) In the list of astronomical instruments which RICHER says that GERBERT used, he mentions only a solid sphere, which was an instrument of Greek, not Arabic, construction. If he had had an astrolabe RICHER would undoubtedly have mentioned it. RICHERUS, *Historia* III, MGH. SS. IV, 617-618.

(15) There is no evidence in the writings of FULBERT that he knew anything at all about an Arabic astrolabe, although PFISTER claims that he knew the Arabic names for the constellations. C. PFISTER, *De Fulberti Carnotensis episcopi vita et operibus*, Nancy, 1885. For a discussion of the work of FULBERT, see L. C. MAC KINNEY, « Bishop Fulbert, » *Isis*, 14, 285-300, 1930. There are no Arabic words in FULBERT's brief notice on the calendar, in MIGNE, *Patrologia latina*, CXLI, 347.

(16) ABBO, the disciple of the astronomer REMI, of St. Germain of Auxerre, wrote on astronomical subjects but there is no trace of an Arabic astrolabe, or Arabic names in his works. He was still relying upon BEDE and EUSEBIUS, and the cycles of CYRILLUS and DIONYSIUS. J. B. PARDIAC, *Histoire de St. Abbon*, M. MANITIUS, *op. cit.*, II, 664-672.

(17) B. PEZ, *Thes. anec. noviss.*, III, II, 37-82, given by him as chapters 14-94. N. BUBNOV, *Gerberti opera mathematica*, Berolini, 1899.

(18) TANNERY and CLERVAL, *op. cit.*, 542.

(19) Might not JOHN OF GORZE have brought back to Lotharingia an astrolabe or a treatise on the subject? *Cf.* Prof. J. W. THOMPSON, *op. cit.*, 188-190. It is important to note that JOHN was in Cordova, a center of Arabic learning. Although there are a number of scientific manuscripts mentioned in an eleventh century catalogue of the abbey of Gorze, none has an Arabic title, D. G. MORIN, Le catalogue des manuscrits de l'abbaye de Gorze au XI^e^ siècle, in *Revue Bénédictine*, 1-14, XXII, 1905.

Although there are one or two very early anonymous treatises on the astrolabe (20), the first ones concerning whose author we have any definite information, are those of HERMANN CONTRACTUS (21), the lame monk of Reichenau. HERMANN, the son of Count WOLVERDS II OF ALSHAUSEN, was born in 1013. About 1020 he was sent to the monastery where he remained during the rest of his life. The « magister » of the monastery school in the time of HERMANN was the great Cluniac scholar BERNO (22). We know practically nothing of the life of BERNO until the year when he succeeded IMMO as abbot of Prüm. That he studied under ABBO OF FLEURY before going to Prüm has been claimed by some but is not at all certain (23). He was a renowned poet, musician, philosopher, and theologian, and his abilities as a scholar and leader finally attracted the notice of HENRY II who in 1008 made him abbot of Reichenau. BERNO took with him to his new monastery many books, and under his influence the monks became extremely interested in music and mathematics. His most cherished student was HERMANN CONTRACTUS whose first enthusiasm for these subjects undoubtedly came from BERNO.

HERMANN probably wrote two treatises on the astrolabe, the first being the *De mensura astrolabii* (24) (c. 1048) which he wrote at the request of his friends who were very curious about this instrument, and especially to please a certain B. who was quite possibly BERNO (25). In this work HERMANN discusses the method of constructing the type of astrolabe called by Latin

(20) L. THORNDIKE, *Magic and experimental science*, I, chapter 30, New York, 1923. N. BUBNOV, *op. cit.*, 370-375.

(21) *Bertholdi Annales*, MGH.SS., V, 267 ff. A. HOLDER, Die *Reichenauer Handschriften*, III, 241-243, Leipzig, 1906-18. M. MANITIUS, *op. cit.*, II, 756-777. M. CANTOR, *Vorlesungen über Geschichte der Mathematik*, 1^3, 885-889, Leipzig, 1913. P. TREUTLEIN, Intorno ad alcuni scritti inediti relative al calcolo dell' abaco in *Bull. di Storia d. Scienze Mat.*, X, 643-7, Rome 1877.

(22) HERMANNUS CONTRACTUS, *Chronicon*, MGH.SS., V, 119. M. MANITIUS, *op. cit.*, II, 61-71.

(23) D. P. BLANCHARD says that BERNO did not study under ABBO, « Œuvres attribuées à Bernon de Reichenau », in *Revue Bénédictine*, XXIX, 98-107 (1912). The story of BERNO and IMMO is important as it shows contact between Reichenau, Prüm and Gorze at this time.

(24) MIGNE, *op. cit.*, CXLIII, 379-390. PEZ, *op. cit.*, III, II, 94-106.

(25) A gloss on Ms. Digby 174, f. 210^v says that this B. was a BERENGARIUS (cf. C. H. HASKINS, *Studies in mediaeval science*, Cambridge, 1927, 53), however it seems much more likely that it was his friend BERNO.

writers Walzachora (26), which is an improved form of the planisphere *(plana sphaera)* described by PTOLEMY in the Almagest (II, 8). At the end of the article, HERMANN says that he intends later to discuss the use of the astrolabe, « Quas autem quantisve utilitates omnes praetaxatae habeant mensurationes libellum astrolapsus legenti, et ingenium in hujusmodi rerum usu exercitanti, alias debet notificari. (27) » This intention seems to have been carried out in the second treatise printed in PEZ, *De utilitatibus astrolabii* (28), although certain chapters cannot possibly be by HERMANN.

In this work HERMANN discusses the value and use of the astrolabe for astronomy and geometry, « his ita praelibatis non pigritia exterritus, de ejus perigraphiis architectonica, seu mechanica ratione comparatis, eorumque nominibus, ne multa scribendo offendam, compendiose dicam, et ut maturius promissa impleam. (29) » The type of astrolabe here described is the astronomical quadrant, which was made by adding a zodiac band, star positions, and scales to the composite quadrant which was a geometrical instrument like the one described in the so-called *Geometria* of GERBERT. The first book of this treatise deals with such problems as the finding of the position of the sun on the ecliptic on any given day, and the positions of the stars at certain hours during any given night. In this (30), as in the other works

(26) E. HONIGMANN says that it comes from wast al-kurra, which is *media pars sphaerae*. He also says that PTOLEMY used only the word planisphere, not astrolabe, which was first used about the time of PHILOPONOS, E. HONIGMANN, *Die sieben Klimata und die Πόλεις ἐπίσημοι*, Heidelberg, 1929. HERMANN's explanation of Walzachora is as follows : « Quicunque astronomicae peritiam disciplinae et coelestium sphaerarum, Geometricaliumque mensurarum altiorem scientiam diligenti veritatis inquisitione altius rimari conatur, et certissimas horologiorum quorumlibet climatum rationes, et quaelibet ad haec pertinentia industrius discriminare nititur, hanc Walzacoram, id est : planam sphaeram Ptolomaei seu astrolapsum sollerti indagatione perquirat et discat, et perquisitam tenaci memoriae firmiter commendet, ita ut cum quaslibet perigraphias partitionesque anaglifarias in ea scriptas et in sculptas cognoverit, singulas nominatim retineat. » PEZ, III, II, 110.

(27) PEZ, *cop. it.*, III, II, 106.

(28) PEZ, *op. cit.*, III, 110-139; also MIGNE, *op. cit.*, CXLIII, 390-403.

(29) HERMANN, *De utilitatibus*, liber I.

(30) This first book has been placed among the doubtful works of GERBERT by N. BUBNOV (*op. cit.*, 109 ff.) but for a number of reasons given in this paper, it seems more probable that it was written by HERMANN.

of HERMANN, many Arabic words are used for the parts of the astrolabe and for the names of the constellations. As HERMANN undoubtedly did not know Arabic himself, he must have used Latin translations containing Arabic words. BUBNOV has printed an anonymous fragment of a manuscript which mentions an astrolabe and contains two or three Arabic words, which he says was possibly in the hand of the author of the *De Utilitatibus* (31). However a comparison of the two treatises shows that this fragment could not have been the main source of the author's information, as it is more in the nature of a general discussion of the value of astronomy, music, and the other subjects of the seven liberal arts, while the *De utilitatibus* gives a very definite and detailed description of the construction and use of an astrolabe and contains many Arabic words.

It is much more likely that HERMANN used copies of Latin translations of the *De constructione astrolabii* and the *De utilitate astrolabii* of MÂSHÂLLÂH as his principal sources (32). So far no Latin translations of the works of MÂSHÂLLÂH have been discovered which were made earlier than the time of HERMANN, but a Latin version of his *Secretorum astrorum* is mentioned in a twelfth century catalogue (33) of the monastery of Wessobrunn which was close to Reichenau. It is possible that this manuscript had been in the library for quite some time before this catalogue was made, and that there were others in this same region. For several reasons it seems more than probable that HERMANN had these translations; first because both his works and those of MÂSHÂLLÂH contain descriptions of the same type of astrolabe, the quadrant astronomical one, and because throughout all the chapters of all the works the same Arabic words are used (34), with identical

(31) N. BUBNOV, *op. cit.*, 109 ff. C. H. HASKINS, *op. cit.*, 51-53.

(32) MÂSHÂLLÂH, d.c. 815-820. G. SARTON, *Introduction to the history of science*, I, 531, Washington 1927. A Latin text of the thirteenth century and an English translation of the Latin has been published by R. T. GUNTHER, *Early science in Oxford* V, 137-192, Oxford 1929. HERMANN's *Liber de utilitatibus*, book 2, chapter 8 is very similar to the chapters 12 and 13 of the *De utilitate* of MÂSHÂLLÂH; and his chapter 11 is similar to chapter 8 of MÂSHÂLLÂH and chapter 7 like chapter 9.

(33) G. BECKER, *Catalogi bibliothecarum antiqui* 231, Bonn, 1885.

(34) For a comparison of the scientific terms used in the anonymous fragment, the works of MÂSHÂLLÂH, and those of HERMANN see appendix below, 198.

spelling, to describe each part of the instrument, and because the same ways of using an astrolabe are given.

The second book of the *De utilitatibus astrolabii* is not a connected treatise like the first but is a collection of writings on various subjects. The first chapter deals with an instrument for telling time whose use HERMANN wishes to explain to a certain brother WERINHAR, presumably the one he mentions in the *Chronicon* as a « Augiensis monachus admodum doctus et vere religioni deditus, » (35) who died about 1053 on a pilgrimage to Jerusalem. Three other chapters (36) in this book take up the question of the circumference of the earth according to the report of MACROBIUS, where HERMANN uses the Archimedean proportional number $\frac{22}{7}$ to calculate the diameter. In addition he employs an indirect method which is also found in chapter 56 of GERBERT's *Geometria* (in the part of the so-called *Geometria* which was probably assembled in Lotharingia), and in a letter of RAGIMBOLD. According to CANTOR, these fractions were not used by BERNELIUS, a student of GERBERT and were not Greek (37), thus we have another link connecting HERMANN and the scientists of Lotharingia. This letter of RAGIMBOLD was probably read by MEINZO OF CONSTANCE (38), a former student of HERMANN, who then sent it to the latter who included it in the *De utilitatibus astrolabii* (book 2, chapter 3). This chapter also shows definite signs of Arabic influence. That the *Geometria* was one of the main sources for this second book is also shown by the fact that HERMANN included two complete chapters in his work, for instance HERMANN's book 2. chapter 6, *Ad mensurandam quamlibet in plano stantem altitudinem per suam ipsius umbram cum quadrato astrolabii*, is word for word like the *Geometria Gerberti*, chapter 21 (PEZ), and his chapter 8, *Alius modus altitudinem rerum metiendi*, is exactly like chapter 82 of the *Geometria*.

The influence of Lotharingian mathematicians and astronomers not only extended southward to Reichenau but also westward

(35) HERMANNUS CONTRACTUS, *Chronicon*, MIGNE, *op. cit.*, CXLIII, 262.

(36) PEZ, *op. cit.*, chapters 2, 3, 4.

(37) M. CANTOR, *op. cit.*, 1^3, 885-889.

(38) M. MANITIUS, *op. cit.*, II, 786-787. For a copy of this letter see E. DÜMMLER, in *Neues Archiv der Gesellschaft für ältere deutsche Geschichtskunde*. V, 202, 206.

to England. From the time of KNUT on through many generations, Lotharingian scholars (39) were very popular in England, and were appointed as archbishops, bishops, and masters of the schools. Before KNUT died, DUDUC, « de Lotharingia oriundus », had already become bishop of Somerset (1033). By the time DUDUC had become bishop of Wells, HERMANN, another Lotharingian, had become bishop of Ramsey, and LEOFRIC who had been educated in Lotharingia was bishop of Exeter (1046-1072). Under EDWARD THE CONFESSOR there was another group of these clerics, for instance ALBERT. All of these churchmen were interested in learning and many brought books with them from their own country.

Earl HAROLD did more than any one to encourage Lotharingian learning in England. He secured the appointments of WALTER as bishop of Hereford (1060-1079) and GISA as bishop of Wells (c. 1060). However his most important appointment was that of ATHELARD OF LIÉGE as the head of the college of canons which he established at Waltham (40). HAROLD had traveled extensively and had discovered that the schools of Lotharingia and the nearby German cities were not only much better than those of England, but also than those of France or northern Italy at that time. It was the system of secular colleges with schools originated by CHRODEGANG of Metz, that HAROLD adopted for Waltham. For head « magister » he chose the learned ATHELARD who had studied in Lotharingia and Utrecht.

During the time of WILLIAM THE CONQUEROR and WILLIAM RUFUS, Lotharingians were still being brought to England. The Conqueror invited to his court ROBERT OF LORRAINE, a distinguished mathematician, who was finally made bishop of Hereford in 1079. He was known for his work on the abacus and more especially because he brought to England the *Chronicon* of MARIANUS SCOTUS upon whose astronomical introduction he himself wrote a treatise. Another interesting scholar of this period was THOMAS OF

(39) For more detailed information on these Lotharingians see especially E. FREEMAN, *Norman Conquest*, 8 vols, Oxford 1867, and T. D. HARDY, *Descriptive catalogue*, 3 vols. London 1871, Rolls Series vol. 26.

(40) FREEMAN, *op. cit.*, II, 451, 600. W. STUBBS, (editor), *De inventione*, IX; C. 15.

BAYEUX (41) who went to Germany and Spain to study the sciences, then after he had spent some time with ODO OF BAYEUX, the latter sent him to Liége to gain still more knowledge. At the time of the Norman Conquest he went to England with ODO and was finally made primate of York in 1070.

Only a few of the many Lotharingian clerics who went to England in the eleventh century have been mentioned, but it is important to note that they were all interested in mathematics and astronomy. However it was not until the time of WALCHER OF MALVERN (42) (c. 1092) that an astrolabe is mentioned in England. WALCHER was also a Lotharingian who arrived in England about 1091. Thus we see that England as well as Reichenau was greatly influenced by Lotharingian mathematicians and astronomers, and that her first hint of Arabic science came from them.

Harvard Library 189 MARY CATHERINE WELBORN.
Cambridge, Mass.

APPENDIX

Glossary of terms used in connection with the astrolabe in the *De mensura astrolabii* of HERMANNUS CONTRACTUS (H), and the *De construtione astrolabii*, the *De utilitate astrolabii* of MÂSHÂLLÂH, (M) and the Anon. Fragment printed by BUBNOV in *Opera geometrica Gerberti* (A).

Term	*Article.*	
Aequales horis (i.e. Muzzenine H)	M	H
Alhabor (i.e. ansa M)	M	
Alhanthabuth (alancabut)	M	H
Allidada	M	H
Almagrip		H
Almenath	M	
Almucantant	M	
Almucantherath	M	H
Almuri (almeri)	M	H
Alnogiza		H
Alpheraz	M	H
Altitudines (i.e. Erufamazeat H)	M	H
Aranea	M	

(41) HUGH THE CANTOR, History of four archbishops of York, in the the *Historians of the church of York*, edited by J. RAINE, II, 98-228, London 1886, Rólls series, 71. W. H. DIXON, *Lives of the archbishops of York*, I, 146-158, edited and enlarged by J. RAINE, London 1863.

(42) C. H. HASKINS, *op. cit.*, 113-117.

Architoph (Alchitob H)	M	H	
Armilla	M	H	
Axis	M	H	
Azimuth	M	H	
Botot altasab		H	
Caballum (Muuses equis)		H	
Camilla	M		
Catezenuel		H	
Cenith	M		
Cimei	M		
Circinum	M	H	
Claves	M		
Climatus	M	H	
Colurus	M	H	
Declinatio	M	H	
Denticulus	M	H	
Diametrum	M	H	
Dorsum astrolabii	M	H	
Equinoctial	M	H	
Eclypsis	M		
Fiducia	M		
Gnomonus	M		
Gradus	M	H	
Intervalla	M	H	
Inaequales horis (i.e. Ezemenie)	M	H	
Latitudine	M	H	
Limbus	M		
Margarite	M		
Margolabrum	M		
Mater	M	H	
Meridianum	M	H	
Nadayr	M	H	
Nadayz	M	H	
Novella	M		
Plaga	M	H	
Plana sphera	M	H	A
Crebra perforate		H	
Tabula perforate	M		
Perigraphiis (= inscriptionum)		H	A
Pinnule	M		
Ptolemy	M	H	
Quadrant	M	H	
Regio	M	H	
Rethum	M		
Septentrionale	M	H	
Spinas	M		
Umbra versa	M	H	
Umbrum	M	H	
Walzacora	M	H	A
Walzazene		H	
Zodiaci	M	H	

Hermannus Contractus
Über das Astrolab

Die älteste Erwähnung des Astrolabs finde ich bei TANNERY (1). Derselbe gibt nach einem Pariser Ms. je vier Briefe von RADOLF DE LIEGE und REGIMBOLD DE COLOGNE. Im fünften Briefe schreibt RADOLF : Astrolabium misissem vobis judicandum, sed est nobis exemplar ad aliud construendum : cujus de scientia, si quid affectatis, ad missam sancti Lan(berti) non vos pigeat advenire. Forsitan non penitebit : alioquin videre tantummodo astrolabium non magis juvabit, quam « lippum pictae tabulae, fomenta podagrum » (finis epistolae deesse videtur). Aus diesen wenigen Zeilen folgt, dass im Jahre 1025 (damals wurde der Brief geschrieben) das Astrolab als etwas ganz Neues eingeführt wurde. Von wem RADOLF das Astrolab geliehen hat, geht aus dem Briefe nicht hervor. TANNERY vermutet von FULBERT DE CHARTRES. Unbestimmt bleibt es auch, ob das Instrument griechischen oder arabischen Ursprungs war. Da aber alle folgenden Erwähnungen das Astrolab mit arabischen Namen verbinden, so ist anzunehmen, dass auch das Astrolab RADOLFS ein arabisches gewesen ist. So berichtet HASKINS (2), dass der Lothringer WALCHER, Prior of Malvern, mit einem Astrolab die Zeiten einer Sonnenfinsternis im Jahre 1092 bestimmte, und dass er bei der Erwähnung des Astrolabs die arabischen Wörter Almagrip, Almeri und Almucantaraz gebrauchte. Es war also im elften Jahrhundert das Astrolab im Abendlande bekannt und im Gebrauch. Es mussten, da man Kenntnis der arabischen Sprache zu der Zeit nicht annehmen kann, Übersetzungen aus

(1) PAUL TANNERY, Mémoires scientifiques, V, p. 229.

(2) CHARLES H. HASKINS, Studies in the history of mediaeval science Cambridge 1924.

dem Arabischen vorhanden sein. BUBNOV (3) macht auf das aus dem zehnten Jahrhundert stammende Pariser Ms. B.N. 17868 aufmerksam, in dem Astronomisches in Verbindung mit arabischen Wörtern enthalten ist. Ob aber auch der Codex Ripoll 225 ebenfalls, wie BEER (4) angibt, aus dem zehnten Jahrhundert stammt, erscheint mir zweifelhaft. Eine Handschrift mit gleichem Inhalt ist Cod. Scal. 38 in Leiden, der aus dem elften Jahrh. herrührt. Dass aber die Schrift *de utilitatibus astrolabii*, die bald GERBERT, bald HERMANNUS CONTRACTUS zugeschrieben wird, vor der Schrift HERMANNS geschrieben ist, erscheint nach HERMANNS Worten in der Einleitung seines *de mensura astrolabii* unzweifelhaft. HERMANNUS sagt, die Kenntnis des Astrolabs sei bis zu seiner Zeit nur confus, dunkel und verstümmelt gewesen. Diese Worte passen sehr gut auf die Schrift *de utilitatibus astrolabii* (zu bemerken, dass der Titel ein späterer Zusatz ist). Namentlich die Sterntabelle ist voller Fehler und Unmöglichkeiten; sie zeigt, dass der Verfasser selbst im Unklaren geblieben ist. Dass diese Arbeit nicht von GERBERT geschrieben ist, dafür gab es bereits viele Gründe (5); der Brief RADOLFS fügt einen ausschlaggebenden hinzu. Hätte GERBERT, der 1003 starb, die Arbeit geschrieben, so wäre sie gewiss 1025 in Köln und Lüttich bekannt gewesen, zumal der Bischof ADELBOLD von Lüttich, der über den Briefwechsel unterrichtet war, ein Schüler GERBERTS war, (HASKINS *l. c.*), dann hätten aber die Worte RADOLFS keinen Sinn. Dafür dass GERBERT das Astrolab gar nicht gekannt hat, spricht der Brief, den er 988 ad ADAM FRATREM schrieb. Hier empfiehlt er zur Bestimmung der Dauer des Solstitialtages den Gebrauch der Clepshydra und erwähnt das Astrolab gar nicht. Ich halte die Schrift für eine Zusammenstellung von Übersetzungen aus dem Arabischen, wie sich aus vielen Ms. ergibt, die Teile der Schrift wörtlich enthalten und geradezu als Übersetzungen aus dem Arabischen angegeben sind. Wer die Zusammenstellung gemacht hat, ist unbekannt.

Auch in der griechischen Welt ist das Astrolab bis in die Zeit

(3) BUBNOV, Gerberti opera mathematica. Berolini 1899.

(4) RUDOLF BEER, Die HS. des Klosters de Ripoll. *Wiener Sitzber. philos. hist. Kl.*, 155. Wien 1907.

(5) LYNN THORNDIKE, A history of magic and experimental science. New York 1927.

des HERMANNUS CONTRACTUS in Benutzung geblieben. Es folgt dies aus einem in Brescia aufbewahrten Astrolab (6) aus dem Jahre 1062, das nur griechische Inschriften und Sternnamen zeigt. Eine Beeinflussung auf das lateinische Abendland aber scheint nicht stattgefunden zu haben. Im Jahresbericht der Erziehungsanstalt des Benediktiner-Stiftes in Maria Einsiedeln vom Jahre 1856/57 (7) steht ein Aufsatz mit dem Titel : Wie man vor tausend Jahren lehrte und lernte. Der nicht genannte Verfasser, ein Benediktinermönch in Maria Einsiedeln, teilt darin ein Tagebuch WALAHFRIDS VON DER REICHENAU (809-849) mit, in dem vom Jahre 825 berichtet wird : Dominus TATTO lehrte uns die Sternbilder, den Tierkreis, die Ursachen der Finsternisse, den Gebrauch des Astrolabs... Da er (WALAHFRID) gleichzeitig mitteilt, dass Dominus WETTIN ihn im Griechischen unterrichtet habe, so wäre anzunehmen, dass das Astolab ein griechisches gewesen sei. Bei einem Besuche in Einsiedeln erklärte mir in freundlicher Weise der Bibliothekar daselbst, dass ein Tagebuch WALAHFRIDS gar nicht existiere, dass vielmehr der Verfasser jenes Aufsatzes rein aus seiner Phantasie geschöpft habe. Da konnte ihm dann leicht ein Anachronismus in die Feder schleichen.

Zu erwähnen ist noch ein *Libellus de Astrolabio*, der zu den zweifelhaften Schriften BEDAS gerechnet wird. (8) Die Schrift kann schon wegen der in ihr enthaltenen arabischen Namen unmöglich von BEDA geschrieben sein. Die grosse Übereinstimmung mit der Arbeit des HERMANNUS CONTRACTUS (beide gebrauchen das arabische Wort Alcotan, das in *de utilitatibus* nicht vorkommt, beide machen bei der Einteilung des Tierkreises den gleichen Fehler) lässt entweder den *Libellus* als ein Fragment der Schrift HERMANNS erscheinen, oder es hat beiden dieselbe unbekannte Übersetzung aus dem Arabischen vorgelegen.

Über Leben und Werke des Mönches HERMANN VON DER REICHENAU, gewöhnlich HERMANNUS CONTRACTUS genannt, dessen Schrift über das Astrolab im folgenden dargeboten wird, gibt Auskunft *Hermanni Contracti vita, seu elogium, a Bertholdo ejus*

(6) O. M. DALTON, The Byzantine astrolabe at Brescia. London 1926. (*Isis*, 11, 170).

(7) Der Katholik N. F. Bd. 16 Mainz 1857.

(8) MIGNE, Patrologia latina, Tom. XC, col. 955.

discipulo scripta, die MIGNE (9) aus *Germaniae sacrae Prodromus Ussermanni* T. I abgedruckt hat. Es wird darin das Astrolab nicht wörtlich genannt, aber es heisst: *In horologiis et musicis instrumentis nulli non par erat componendis;* das Astrolab wurde zu den instrumenta horologica gerechnet. Dass die später *De mensura Astrolabii* betitelte Arbeit von HERMANNUS geschrieben ist, hat niemand bezweifelt, seit man weiss, dass es bereits im zehnten Jahrhundert lateinische Übersetzungen von astronomischen arabischen Schriften gab. Die Arbeit findet sich in mehr als 30 Handschriften, die zum grössten Teile dem XII. Jahrh. entstammen. (Man findet sie aufgezählt bei BUBNOV und HASKINS). PEZ (10) hat sie nach dem Salzburger Codex a.V. 7. gedruckt, MIGNE darnach abgedruckt. Dieser Codex ist zwischen 1127 und 1160 geschrieben (11). Der Druck enthält neben Fehlern, die im Codex stehen, auch eigene. Von den Ms. aus dem XI. Jahrh. wähle ich das wohl älteste und beste, nämlich den Münchener Cod. 14836, für dessen gütige Überlassung ich auch an dieser Stelle der Bibliotheks-Verwaltung gebührenden Dank ausspreche. Die Arbeit HERMANNS in diesem Codex ist in der ersten Hälfte des XI. Jahrh., also noch zu Lebzeiten HERMANNS geschrieben. (12) Von den Kapitelüberschriften sind in der Handschrift nur zwei genannt *(de lineis horarum* und *de alhancaput);* ich habe die übrigen mit der Bezifferung aus dem Salzburger Codex hinzugefügt, ebenso die Figuren zu den Kapiteln I, II, IX.

***

Hermannus Christi pauperum peripsima, et philosophorum tironum asello immo limace tardior assecla. B. suo jugem in Domino salutem.

Cum a pluribus saepe amicis rogarer, ut mensuram astrolabii, quae apud nostrates confusa, obscura et passim mutilata vulgo invenitur, lucidius pleniusque scribere temptarem, idque hactenus tum propter inscientiam, tum etiam propter pedissequam, heu mihi familiarem desidiam subterfugerem, tandem a tui potissimum

(9) MIGNE, Patrologia latina, Tom. CILIII, col. 109-130.
(10) PEZ, Thesaurus Anecdotorum Noviss. T. III, pars 2 (1721), col. 109-130.
(11) M. CANTOR, Vorl. z. Gesch. d. Math. 4. Aufl. 1922, I, Seite 859.
(12) M. CURTZE, in der *Zeitschr. f. Math. u. Phys.* Bd. 40.

instantia perpulsus, prout inertia pigritiaque permittente potui diu seposito more mendacium fabrorum operi cunctabundam deses (*) manum applicui, non lepori, non eleganti verborum compositioni, sed intricatae rei aperte studens explicationi. In qua re, si quid indocta obsequentis peccavit temeritas, benigna jubentium amicorum indulgeat caritas, totumque suo potius delictum asscribat studio, quae imbecilli tam grave non pepercit onus imponere dorso.

Tu igitur, tu anime mi, utcumque alii ferant mea scripta legentes, gratanter, ut amicum decet, quomodocumque descriptam simplicem praefati instrumenti accipe mensuram. Ut si te postmodum de ejus ratione cognovero permoveri, ea tecum alacrius, suppetente alias otio, pro posse studeam vestigare.

CAP. I. *Quomodo circuli Aequinoctialis, colurorum solstitiorumque in astrolabio inveniendi sint.*

In metienda igitur subtilissimae inventionis Ptolomei waltalchora, id est plana sphaera, quam astrolabium vocitamus, tres primum circulos invenire debemus. Quorum medium, qui aequinoctialis coelestis circuli vice nomen obtinet, juxta quantitatem tabulae, ne maximum dumtaxat praepediat, ad libitum moderari convenit. Quem centro A diligenter circumscriptum in quatuor aequa partiaris punctis B. C. D. E., et sit B a sinistris orientale punctum, C huic oppositum a dextris occidentale, D inferius septentrionale, E superius meridianum. Dehinc, apposita regula per B.A.C. puncta, rectam lineam a fine ad finem tabulae deducito, id est, ab oriente in occidentem. Itemque per D.A.E., id est, a septentrione in meridiem alteram lineam. Quas videlicet lineas quadrifariam dividentes arabes alcot nominant, nos curulos. His ita positis unumquemque circuli quadrantem in XC aequas partes dividere, et ex his XXIIII ab utroque latere C versus D et E debes accipere. Sed ut hoc facilius fiat, quadrantes singulos in tria divide, ut scilicet unaquaeque tertia XXX partes capiat, et tertias duas C contiguas in V singulas partire, unamquamque quinarum partem VI de XC partibus subintelligens claudere. Ex his quinque partibus IIII utrinque circa C secerne et punctis superius F, inferius G inscribe. Sicque inter C et F itemque

(*) Im MS. steht dese r. Das weiss ich nicht zu deuten, setze daher in Übereinstimmung mit PEZ deses.

inter C et G XXIIII partes de XC inclusas noveris esse. Postea in puncta E et F. vel, si mavis, D et G posita regula vide, in quo loco lineam ab oriente in occidentem per B.A.C. ductam extra

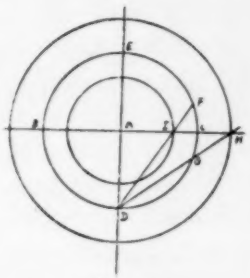

Fig. 1

circulum tangat, ibique nota H. impressa, circini pedes in A. et H. figito, et latiorem solstitii hiemalis circulum centro A et intervallo H circumducito. Item in puncta E et G, sive D et F regulam ponens, et ubi lineam praedictam B.A.C. tangit, notam I infigens, nihilominus fixo in A et I circino, minimum aestivi solstitii circulum centro A circinato.

CAP. II. *De almucantarat.*

His circulis ita inventis, quodcunque clyma construere velis, ejus clymatis altitudinis quot sint gradus vide, utpote per primi climatis XV, secundi XXIII, tertii XXX, quarti XXXVI, quinti XLI, sexti XLV, septimi XLVIII, et tot partes de XC a C versus E secerne, itemque ab opposito a B versus punctum D totidem; verbi gratia, si nostrum, id est septimum, clyma volueris metiri, sume a C versus E XLVIII partes de XC superiores videlicet XXIIII, per quas circulos inveneras duplicatas, easque nota K finito. Item a B versus D tantumdem, id est XLVIII partes nota L secernito, ut videlicet integra circuli medietas a K ad L inveniatur. Postea emiciclum a K per E et B usque ad L in VI partire et unamquamque sextam in VI itidem punctis impressis partire, et hoc facto habetis a K usque ad L intervalla XXXVI, singula quinas partes de CCCLX totius circuli partibus complectentia. Deinde regulam in C et K puncta ponens, vide, quo in loco tangat lineam meridianam per D.A.E. longe extra circulos in alia tabula, super quam wahtalchora metienda ponitur, prolongatam, ibique M notam scribe, sicque, non mota hinc, id est de C puncto regula, ad singula per ordinem, quae a K

ad L feceras puncta, eamdem regulam ponito, et ad singulas positiones praefatam D.A.E. lineam, ubi a regula tangatur annotato, donec tricies et sexies ipsa linea notata in C et L puncta regulam

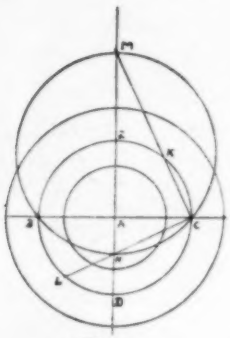

Fig. 2

ponas et ubi saepe dictam tangat lineam, trigesimum septimum signum, id est N facias. Quo facto spatium lineae, quod inter M et N continetur, accepto circino in duo divide, et in medietate centro fixo emiciclum, quem almucantarat arabes nominant, inter tres circulos per B.N.C. inflecte. Iterum spatium lineae, quod inter proximum signum ab M et proximum ab N complectitur, in duo dividens et, centrum in medio figens, alterum itidem emiciclum curvabis. Eodem modo spatii inter lineae tertium ab M et tertium ab N signum inventa medietate, tertium emiciclum debes incurvare; sicque ordinatim per singula ejusdem lineae signa interius utrinque progrediens, spatiumque, quod inter ipsa continetur, in duo dividens, nunc semiplenos, nunc integros circulos circinabis, donec ad medium signorum perveniens, octodecim orbiculorum almucantarat consumabis, sicque ab N., id est a primo almucantarat usque ad medii orbiculi centrum XC gradus computantur, per singula scilicet ampilogrammorum intervalla quinis gradibus enumeratis. Ab ipso vero N usque ad medium centrum A tot gradus in unoquoque clymate reperis, quot gradus ipsius, quod mensurabis, habet altitudo clymatis. Utpote in septimo, cujus nunc mensurae exemplar expressi, XLVIII.

CAP. III. *De lineis horarum.*

Hoc modo perfectis almucantarat ampilogrammis spatium trium primitus dictorum circulorum, quod infra primum almucantarat

ex parte scilicet D, id est septentrionali, remanet, XII horarum locationi aequaliter distribui debet hoc modo : Initio ab ipso almucantarat a parte C sumpto, spatium cujusque trium circulorum usque ad lineam D.A.E sex antemeridianis horis aequaliter dispertire. Similiter ab ipsa D.A.E linea usque ad almucantarat ex parte B sena in singulis circulis metiens intervalla postmeridianis horis deputabis. Postea circini accepti unum pedem tali in loco figere debes, ut alter tria puncta primae horae limitem in singulis scilicet circulis signantia possit attingere, et ita circinando finalem primae horae lineam inter ipsos tres circulos curvabis. Eodemque modo per singulas horas faciens nunc contrahendo, nunc dilatando in diversis locis circinum, prout punctorum positio poscit, figendo, singularum horarum finales lineas circinabis; sicque singulis horis nascentur intervalla ad interiorem quidem circulum contractiora, ad exteriorem vero latiora, quae videlicet ipsa septentrionalis D.A.E in medio limitat linea. Talibus ita mensuris et perigraphiis quaternas VII scilicet clymatum tabellas insignito.

CAP. IV. *De alhancaput.*

Deinde volvellum seu rete, quod arabes alhancaput nominant, crebra pertusione foratum, hoc est modo metiendum. Descriptis primo in tabula ejus aliquantum spissiora, tribus circulis ejusdem, cujus et supra quantitatis aequinoctiali videlicet, hiemali atque aestivo solsticialibus, eodemque modo lineis rectis, quas coluros dixi, in quatuor aequa, ut supra, divisis, colurum unum versus meridiem a septentrione directum a puncto, ubi aestivum minimum secat usque ad punctum, ubi ex opposito jungitur maximo hiemali circulo, in duo divide et centro in medio fixo zodiacum circulum, in quo tota hujus artis consistit efficacitas, ita circumscribe, ut minimum totum infra se continens circulum ex una ei parte conjungatur, ex opposita per diametrum parte maximo adunetur, mediumque circulum altrinsecus simul et alium colurum ad singula puncta intersecet, atque per ipsius coluri lineam in bina emisphaeria, sed non aequaliter dividatur; ipsi quoque congruam latitudinem, in qua nomina duodecim signorum et quarundam stellarum inscribi possint, ad libitum tribue. Spatia vero et loca ipsorum duodecim signorum hoc modo invenias.

CAP. V. *De spatiis signorum inveniendis.*

Maximum tertium circulorum in XII partire, unumquemque scilicet quadrantem in tria. Dehinc regulam rectam ad singulas

XII partes et ad trium circulorum centrum ponito, et zodiacum, quem feceras circulum, ubi ad singulas positiones a regula tangitur, lineis secato, et ita XII in eo spatia sed non aequalia efficiens in minoris emisphaerii senis spatiis sena signa ab ariete usque libram contra mundum a dextra scilicet in sinistram ordinato. In majoris quoque totidem spatiis totidem alia a libra usque ad arietem signa. Sicque ab aequinoctiali circulo ad aestivum aries, taurus, gemini pertendunt; indeque rursum ad aequinoctialem cancer cum leone et virgine reclinantur. Porro idem ad hiemalem libra, scorpius sagittariusque devergunt; et inde itidem ad aequinoctialem capricornus cum aquario et piscibus resurgunt. In confinio quoque sagittarii et capricorni quidam prominet denticulus almeri arabice, id est calculator vocitatus. Ad partes vero sive gradus ipsorum signorum in declivo ejusdem zodiaci latere discernendos praefatas XII maximi circuli partes in VI item singulas partes partire, et ad singulas partiones ipsas atque ad centrum trium circulorum regulam, ut supra, ponens, et zodiacum circulum, ubi a regula tangitur, signans in singulis XII signis sena, in toto vero zodiaco LXXII efficies intervalla, singula quinos de CCCLX totius circuli gradibus complectentia.

CAP. VI. *De positione stellarum.*

Hoc modo zodiaco perfecto acutos claviculos sparsim certis in locis in alhancabut fixos, quorum acumina clarissimarum quarundam stellarum vicem tenent, hac ratione ponendos docent. Quamcunque ergo stellam in eo locare volueris, prius in pagina subscripta contra quem gradum cujuscunque XII signorum eadem poni debeat praevide; et in eumdem gradum atque in trium circulorum centrum regulam ponens, mediam ejusdem denticuli seu stellae lineam dirige, Postquam ab eo gradu, quo maximus a regula circulus tangitur, tot gradus in eodem maximo circulo, qui in CCCLX divisus est, iterum numera quot eidem stellae in eadem ascriptos videris pagina, et ubi numerus ipse utrinque finitur, ibi altrinsecus regulam pone, atque ubi stellae linea a regula tangitur, ibi acumen porrectionis ejus abscindetur : v.g. Alramech in zona Bootis fixam posita in vicesimo quarto gradu librae et in centro trium circulorum regulam lineabis et ab eo gradu in maximo circulo LXV gradus utrinque numerans, ubi ipse numerus terminatur, apposita regula acumen Alramech juxta eam finem sortiatur. Eodem modo juxta numeros in ipsa pagina

singulis stellis asscriptos post alramech in ipso zodiaco Elfeca, Alhamum, Altahir Delfoi, et Alferaz locentur. In parvulo ducto Benenath, Wega, Alrif, Algol, Altaich, Egregez et Alrugaba ordinantur. In parte vero aequinoctiali circuli a regione Tauri usque ad Virginis regionem ad hoc nihilominus relicta Aldebaran Algenze, Algomeisa, Aldiraan et Calhadizeda statuuntur. In maximo autem hiemali circulo qui ad hunc itidem usum ab Aquario usque ad Sagittarium in extremis volvelli terminis relinquitur, Debalgedi, Denebcaitoz, Patancaitos, Rigel, Alhabor, Algurab, Alcimech et Calbalagrab, quamvis non adeo utiles, utpote in Austro juxta horizontem diverse ponantur, ut praefata pagina subtus asscripta docet.

1)	a	XXIIII	LXV	Libra	Almarech
2)	a	XVI	LXXI	Scorpius	Elfeca
3)	a	XXIIII	LVII	Sagitta	Albaum
4)	a	XIIII	LIIII	Capricor.	Altahir
5)	a	XXV	LIX	Capricor.	Delfin
6)	a	XXX	LXV	Aquarius	Alferaz
7)		XII	CXI	Taurus	Albadib
8)	b	XIX	LXXIII	Capric.	Alrif
9)	b	I	LXXII	Capric.	Wega
10)	b	XVIII	LXXIIII	Libra	Benenaz
11)		XXV	XIIII	Scorpius	Calbalagrab
12)	d	VIII	XV	Libra	Alcimech
13)	d	XVIII	XLI	Virgo	Algurap
14)	d	I	XXXVI	Cancer	Alhabor
15)	d	VIII	XXXIX	Gemini	Rigel caitoz
16)	d	XX	XXXVI	Aries	Caitozpatan Denebcaitoz
17)	d	XX	XXXV	Pisces	Caitozdeneb vel Denebalgrata
18)	d	VIII	XIX	Aquarius	Liechdeneb vel Pantancaitoz
19)	c	XXVII	LXI	Taurus	Aldebaran. c
20)	c	XVIII	LV	Gemini	Maleuxe vel Algomeisa
21)	c	XVII	LVI	Cancer	Algeuze . c
22)	c	VI	LII	Leo	Aldiraan . c
23)	c	XVIII	LXI	Leo	Calbalazeda . c
24)	b	X	LXXI	Taurus	Algol b
25)	b	III	LXXIIII	Gemini	Alhaich b
26)	b	XIX	LXXV	Leo	Alrucuba b
27)	b	XXVI	LXXVI	Cancer	Egregez b

Hoc modo alhancabut mensuratum totum, ubicunque vacat diligenter facias perforari et perpoliri; zodiaco et partibus aliorum, quos dixi, cum stellis suis circulorum et quadam regula per pisces et virginem secundum alium colurum ducta, quae ipsos videlicet cum medio centri circulo possit continere ad ejus soliditatem relictis, ut cum alicui superpositum clymati per axem, quem alchitob vocant, circumvolvitur apparentibus per apertum cunctis ejusdem clymatis perigraphis, ortus, occasus exaltationesque signorum et stellarum ratioque horarum liquido declaretur, sicut per ipsius artis experientiam et usum manifeste scire volentibus approbatur.

CAP. VII. *De partitione umbonis.*

Hoc cum ita perfecto umbonem, qui ipsam matrem Wabtacore circumdare, et cunctas clymatum tabulas ipsumque superpositum alhancabut debet circumcludere, primo in quatuor aequa, ut videlicet praefatis conveniat coluris, divide. Dehinc unumquodque quartorum in tria, et horum unumquodque in sena partire, ut videlicet totus umbonis circuitus lineis rectis distincta LXXII paria contineat intervalla, et unumquodque horum intervallorum interius, ubi prominens in alhancabut almeri adtingit, in quinos adhuc gradus discerne, in quibus circumiens almeri dierum et horarum longitudines possit computare.

CAP. VIII. *De partitione signorum, mensium et dierum.*

Restat postica Wabtalcore planities metienda omnibus zodiaci gradibus, signis, mensibus et diebus anni insignienda. Cujus extremos circulos in quatuor itidem aequa rectis lineis per centrum ductis divisos eadem qua et praedictum umbonem mensura distinguas, ut videlicet unusquisque quadrans (primum in tria et illa) in sena, id est XVIII intervalla, totus vero circuitus in LXXII divisus sit, et unumquodque horum in quinos dividatur gradus, qui simul CCCLX complebunt. Sub his vero proximum circulorum intervallum tribus in unoquoque quadrante signis dispertiatur eorumque vocabulis inscribatur. A linea videlicet occidentali, usque ad summam australem, cui suspendicularis incantenatur circulus contra mundum Aries, Taurus et Gemini. Abhinc usque ad orientalem Cancer, Leo et Virgo. Inde usque ad imam septentrionalem Libra, Scorpius et Sagittarius. Et dehinc ad occidentalem Capricornus, Aquarius et Pisces convertantur; et unicuique signo sexies quini, id est XXX de praedictis CCCLX totius

circuli gradibus deputentur. Sub hoc item aliud circulorum intervallum mensibus duodecim a medio fere signorum ad se pertinentium inchoantibus, distribuatur eorumque nominibus insigniatur. Et sub item aliud diebus anni CCCLX vel potius V adhuc additis dividatur : quod hoc modo facilius facies : Ab uno circuli quadrante decimam octavam partem ejus seclude. Et hac excepta reliquum totum circuitum in XII, et unamquamque duodecimam in senas divide partes, sicque annumerata, quam secluseras quadrantis (parte) decimam octavam in toto circulo LXXIII reperies intervalla, quibus singulis in V partitis habes circulum per CCCLXV dies divisum. Tunc (a) vernali aequinoctio, quod moderni primo gradu Arietis, id est XV kal. Aprilis ascribendum credunt, incipiens XIIII dies post aequinoctium, id est post lineam occidentalem, in qua arietem inchoasti, Martio mensi tribue, deinde Aprili XXX, Majo XXXI, Junio XXX, Julio XXXI, Augusto XXXI, Septembri XXX, Octobri XXXI, Novembri XXX, Decembri XXXI, Januario XXXI, Februario XXVIII et Martio usque ad praefatam aequinoctii vernalis lineam XVII deputans. Integrum anni currentis circuitum complebis, duabus lineis circulos quadrifariam partientibus scilicet quaternos anni articulos, bina scilicet aequinoctia et totidem solstitia intersecantibus. Adhuc quod sub hoc remanet centro citimum circulorum intervallum ita in uno quoque mense distingue, ut quinos dies, vel quod expeditius propter hebdomadas videtur, septenos potius commemoratos lineis distinguas, remanentibus si quid adhuc supersunt de mense diebus. His etiam ita perfectis totam mensuram instrumenti hujus ad astronomicam videlicet horologicamve disciplinam pertinentem completam esse laetaberis. Sed adhuc de quadrantis mensuratione, qui intra circulum dierum inter occidentalem et septentrionalem lineam versus medium aquarium locatus ad geometricas dimensiones utilissimus pertinet, paucula scribere non pigere debet.

CAP. IX. *De quadrato.*

Quartam ergo dierum circuli partem inter praedictas occidentalem et septentrionalem lineas in duo divide et puncto A in medio facto, aliam quoque quartam inter septentrionalem (es müsste heissen : occidentalem) et australem, et item aliam inter septentrionalem et orientalem in duo itidem dispertiens medietates punctis B et C notato. Tunc apposita regula in puncto A et B lineam rectam ab occidentali linea in A dirige; item in A et C

regulam ponens aliam lineam a septentrionali linea item in A deducito, et habes quadratum orthogonum aequi laterum, cujus unus angulus in A, alter oppositus per diagonum in centro, qui

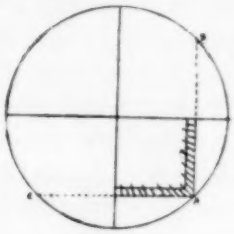

Fig. 3

reliqui in lineis occidentali et septentrionali limitantur. Hujus duo latera angulum in A facientia, data eis ad libitum latitudine, in duodecim aequales partes divide, et interius duas ex his complectens in sena partire, quatenus supercurrente adhidada, id est quadam regula cum duabus pinnis erectis, ad rectam lineam perforatis, quota lateris quadrati pars designetur possit apparere. Quas autem quantasve utilitates omnes praetaxatae habeant mensuratione, libellum astrolapsus relegenti et ingenium in hujusmodi rerum usu exercitanti alias debet notificare.

* * *

Die Erklärung der aus dem Arabischen stammenden Wörter verdanke ich den freundlichen Mitteilungen des Herrn Prof. RUSKA. Die Sternnamen finden sich, mit Ausnahme von Egregez auf dem in Berlin befindlichen arabischen Astrolab aus dem Jahre 1029. (13) Herr Prof. HÖLSCHER, Marburg, hatte die Güte, mir die arabisch gedruckten Sternnamen zu lesen. Einiges entnahm ich auch der Arbeit IDELERS (14).

Waltalchora (walzagora, Wazalchora u. ähnl.) = waḍ'at al-kurra.
Alcot (alcotan) = al-qâṭi'ân, die beiden Schneidenden.
Almucantarat = muqanṭarât, die Brücken.
Alhancaput = al-'ankabût, die Spinne.
Almeri = al-mûrî, der Rechner.
Alchitob = al-quṭb, die Axe.

(13) WOEPCKE, Über ein in d. K. Bibl. zu Berlin befindliches arabisches Astrolabium. in *Math. Abhandl. d. K. Akad. d. Wissensch. zu Berlin* aus dem Jahre 1858.

(14) IDELER, Untersuchungen über den Ursprung u. d. Bedeutung der Sternnamen. Berlin 1809.

Die Sternnamen.

1) Alramech, Almarech = al-simâk al-râmiḥ, der Simak mit der Lanze.
2) Elfeca, Elfeca vel mimir = al-nîr min al-fekka, der Helle in der Bettlerschüssel.
3) Alhaum = ra's al-ḥâwî, Kopf des Schlangenträgers.
4) Altair = al-ṭâ'ir, der Vogel.
5) Delfin, Delfoi = al-dulfîn.
6) Alferaz, Alferat = mankib al-faras, die Schulter des Pferdes.
7) Albadib, Alhadip, Alhapit, Alhadab, Alhidib, Alhadid, Allutib, Alhadab = al-kaff al-khaḍîb, die gefärbte Hand.
8) Alrif = al-ridf, der Nachfolger.
9) Wega = al-nasr al-wāki', der fallende Adler.
10) Benenaz, Benenath, entstanden aus na'sh, die Bahre und el-benât, die Töchter.
11) Calbalagrab, Calbalagrat = qalb al-'aqrab, Herz des Scorpions.
12) Alcimech, Alchimech = al-simâk al-'a'zal, der unbewaffnete Simak.
13) Algurab, Algurabi = janâḥ al-ghurâb, Flügel des Raben.
14) Alhabor = al-shi'râ al-'abûr, Sirius.
15) Rigel = rijl al-jawzâ' (oder al-jabbār), der Fuss des Gausa.
16) Caitozpatan = baṭn al-qaiṭâs ((= κῆτος), Bauch des Walfisches. Denebcaitoz gehört zu 17).
17) Caitozdeneb = dhanab al-qaiṭâs, der Schwanz des Walfisches. Denebalgrata gehört zu 18.
18) Liechdeneb, Libidenopi, Libidineh, Denebalgedi = dhanab al-jadî, Schwanz des Steinbocks pantakaitoz gehört zu 16.
19) Aldebaran, Anabajedi von dabar = successit, weil er den Plejaden folgt.
20) Maleuxe, Algenxe, Malerixe = mankib al-jawzâ', Schulter des Gausa. Algomeisa zu 21.
21) Algeuze, Algomeisa = al-shi'râ al-ghumaiṣâ', die Triefäugige.
22) Aldiraan, Aldiran = mûkdim al-dirâ'ain, das Vorderste von den Armen.
23) Calbalaceda, Caldata Zedapes = qalb al-asad, Herz des Löwen.
24) Algol = ra's al-ghûl, Kopf des Gul.
25) Alhaich, Halhazhoch, Altaich = al-'aiyûq, die Ziege.
26) Alrucuba = rukbat al-dubb, Knie des Bären.
27) Egregez, Egreget = yad al-dubb, die Hand des Bären. Für

den Namen finde ich keine Erklärung; er kommt auch in der Tabelle zu CHAUCERS Schrift vor. (15)

Ob das am Ende des 2. und Anfang des 3. Cap. stehende Wort ampilogrammi eine arabische Wurzel enthält, kann ich nicht beurteilen; in anderen MS. steht campilogrammi. (15*a*) Da das Wort offenbar Kurve, Kreis bedeuted, so steckt vielleicht, trotz der falschen Bildung campilo... statt campalo..., das griechische *καμπαλέος* = krumm darin. Im DU CANGE finde ich das Wort nicht.

Zu Cap. V. Bei der Einteilung des Tierkreises macht HERMANNUS einen Fehler. Er teilt den Winterkreis in 12 gleiche Teile, verbindet die Teilpunkte mit dem Mittelpunkt des Astrolabs und sagt, diese Verbindungslinien teilen den Tierkreis in die 12 Zeichen. Die Teilungslinien schliessen also je 30 Grad des grössten Kreises ein. In Wirklichkeit aber fallen auf Widder, Jungfrau, Wage, Fische je 27° 54', auf Stier, Löwe, Scorpion ie 29° 54', auf Zwillinge, Krebs, Schütze, Steinbock je 32° 12'. Die Linien sind nämlich nach den Rectascensionen der Zeichenanfänge zu ziehen. Die HS., die die Sterntabelle ohne Zusammenhang mit der Schrift des HERMANNUS bringen, Ripoll 225, Leiden, Scaliger 38, Vat. Reg. lat. 598, Vat. Palat. 1577, Vat. lat. 1661, Vat. Palat. 1356, (die ersten 6 aus dem XI, die siebente aus dem XII Jahrh.) sagen richtig : Tene, ut ad metienda in zodiaco intervalla signorum numeres ab utraque parte meridianae XXXII gradus in exteriori umbone, ab australi XXVII (statt australi ist aequinoctiali zu lesen), dann heisst es weiter : Taurus ab ariete tribus gradibus superatur (das soll heissen : taurus wird erhöht um drei Grad bezüglich des Aries), a tauro etiam gemini, sic et in alia parte leo a virgine et cancer a leone. Vel sic primus gradus leonis a XXXIII gradibus exterioris circuli sumitur, virginis a LXIII gradibus (incisio) medii circuli; in rete LXVI extimi umbonis.

Der gleiche Fehler steht noch im libellus de astrolabio, das fälschlicherweise BEDA zugeschrieben wurde. Es scheint mir vielmehr, dass der libellus ein kleines Fragment der HERMANNUS-arbeit darstellt.

Da nun aus der Schrift de *horologio viatorum*, die unzweifelhaft

(15) R. T. GUNTHER, Early science in Oxford, V, 1929 (*Isis*, 14, 233-35).

(15*a*) *Καμπύλη γραμμή* = gekrümmte Linie. Aussprache : campiligrammi. (A. POGO.)

HERMANNUS CONTRACTUS zum Verfasser hat (15*b*), hervorgeht, dass HERMANNUS selbst ein Astrolab in Händen hatte, so müsste dies denselben Fehler gehabt haben, oder, was wahrscheinlicher ist, HERMANNUS hat sich an dieser Stelle eine Flüchtigkeit zu schulden kommen lassen. Jedenfalls aber haben ihm die genannten Schriften nicht vorgelegen. Die Übersetzung, die allen bisher bekannten MS. zugrunde lag, ist ebenso wie das arabische Original bis heute unbekannt.

Zu Cap. VI. In der Sterntabelle bedeutet die erste Zahl in Verbindung mit dem an dritter Stelle stehenden Tierkreiszeichen die Stelle des Tierkreises, die mit dem Stern gleichzeitig durch den Meridian geht, also die im Mittelalter viel gebrauchte Mediatio coeli. Die zweite Zahl gibt die Anzahl Grade θ an, die vom Schnitt der Mediatiolinie mit dem Winterkreise auf letzterem nach beiden Seiten abzutragen sind. Die Verbindungslinie der Endpunkte der Abtragungen trifft die Mediatiolinie im Sternpunkte. Da der Winkel θ durch Beobachtung nicht zu ermitteln ist, nehme ich an, dass er an einem vorliegenden Astrolab abgelesen wurde. Dieselbe Bestimmungsart fidet sich im MS. Cambr. Univ. lib. Hh. 6. 8, fol. 236 unter der Bezeichnung : Gradus longitudinis ex utraque parte; hier ist daneben noch die Altitudo meridiana für Paris angegeben, freilich stimmen die nach beiden Angaben errechneten Declinationen sehr schlecht überein. (16) Die Araber bestimmten den Sternpunkt durch Rectascension und Declination, oder durch Länge und Breite, wie es schon PTOLEMAIOS angegeben hat. (17) Aus den Angaben HERMANNS erhält man die Declination, wie folgt : In der Figur 4 sei CD die Linie

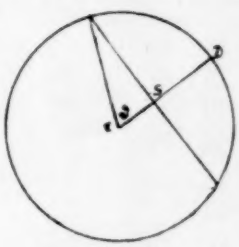

Fig. 4

(15*b*) E. ZINNER, Horologium viatorum (*Isis*, 14, 385-87, 1930).
(16) W. W. SKEAT, The Astrolabe by GEOFFREY CHAUCER. London 1872.
(17) *Isis*, 9, 278, 1927.

der Mediatio coeli, CD der Radius des Winterkreises, CS der Radius des Declinationkreises. Ist nun der Radius des Aequators gleich 1, so ist CD = tang (45° + 12°), CS = tang (45°—½ δ); da nun CS = CD. cos θ, so folgt tang (45° — ½ δ) = tang 57°. cos θ. Aus dieser Gleichung erhält man die Declination. Aus der Mediatio coeli μ und der Declination θ erhält man die Breite β und die Länge λ in folgender Art : In der Figur 5 sei ARQ

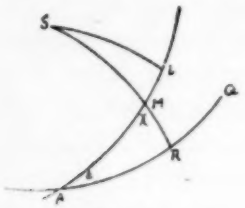

Fig. 5

den Aequator, AML der Tierkreis, SR die Declination δ, SL die Breite β des Sternes S, AM = μ (Mediatio coeli) und der Winkel MAR die Schiefe der Ekliptik ε, dann ist, da beide Dreiecke bei R und L rechtwinkelig sind, sin MR = sin μ. sin ε, cot χ = cos μ. tang ε, ferner SM = δ — MR, sin β = sin SM. sin χ, tang ML = tang SM. cos χ und λ = μ + ML. Die Werte der folgenden Tabelle sind nach diesen Gleichungen aus den Angaben des Münchener Cod. 14836 errechnet. Hinzugefügt sind die Positionen bei Ptolemaios, die Abweichungen und die heutigen Sternnamen.

	MS.		Ptolemaios		Differenz		
	λ	β	λ	β	λ	β	
1) Almarech	190°	31°	177°	31°	13°	0°	α Bootis.
2) Elfeca	204	50	191	47	13	3	β Coronae bor.
3) Alhaum	262	33	235	36	27	3	α Serpentarii.
4) Altahir	287	29	274	29	13	0	α Aquilae.
5) Delfin	302	34	290	34	12	0	α Delphini.
6) Alferaz	344	33	332	31	12	2	β Pegasi.
7) Albadib	50	24					
8) Alrif	320	60	309	60	11	0	α Cygni.
9) Wega	271	62	257	62	14	0	α Lyrae.
10) Benenaz	173	46	150	54	23	8	η Ursae maj.
11) Calbalagrab	236	— 3	223	— 4	13	1	α Scorpionis.
12) Alcimech	190	— 5	177	— 2	13	3	α Virginis.

13) Algurap	174	—12	163	—15	11	3	γ	Corvi.
14) Alhabor	91	—39	77	39	14	0	α	Canis maj.
15) Rigel	62	—32	50	—32	12	0	β	Orionis.
16) Caitospatan	12	—19	355	—20	17	1	ζ	Ceti.
17) Caitosdeneb	347	— 9	334	— 9	13	0	ι	Ceti.
18) Liechdeneb	307	— 2	295	— 2	12	0	δ	Capticorni.
19) Aldebaran	55	— 4	42	— 5	13	1	α	Tauri.
20) Maleuxe	76	—16	62	—17	14	1	α	Orionis.
21) Algeuse	103	—15	89	—16	14	1	α	Canis min.
22) Aldiraan	130	—15						
23) Calbalazeda	138	1	123	0	15	1	α	Leonis.
24) Algol	47	20	30	23	17	3	β	Persei.
25) Alhaich	68	23	55	23	13	0	α	Agitatoris.
26) Alrucuba	128	29	114	28	14	1	μ	Ursae maj.
27) Egregez	110	27	96	29	14	1	ι	Ursae maj.

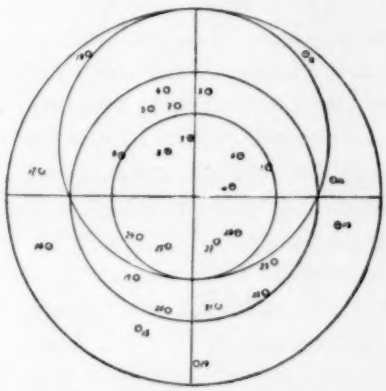

Fig. 6 Spinne des Astrolabs.

Für No. 7 geben alle HS. Taurus 12. Ein Blick auf die gezeichnete Spinne, auf der eine gewisse Ordnung der Sterne unverkennbar ist, lässt diese Angabe unwahrscheinlich erscheinen, verlangt vielmehr den Stern in Gruppe 4 bis 9. Nun finde ich in mehreren HS. neben Alhapit die nähere Angabe « in telo » oder « media in telo ». Das Sternbild Telum gehört aber zum Capricornus. Aus diesen zwei Gründen halte ich Taurus für ein Versehen und setze dafür Capricornus. Die Altitudo ist in verschiedenen HS. verschieden angegeben. Unsere HS. hat CXI, ich halte das für ein schlecht geschriebenes LXI. Mit diesen Zahlen erhält man $\lambda = 286^{0}$, $\beta = 39^{0}$. PTOLEMAIOS hat

für β Teli $\lambda = 273°$, $\beta = 39°$, die Differenz wird demnach 13°, 0°. Die oben gegebene Erklärung des Wortes Alhapit = al-kaff al-khaḍīb, das ist β Kassiopejae, fällt dann freilich fort, aber β Kassiopejae passt gar nicht zu der angegebenen Position.

Bei No. 10 ist der Unterschied gegen PTOLEMAIOS ganz ungewöhnlich gross. Ich vermute einen Schreibfehler, θ = LXXIIII an Stelle von θ = LXXVIII. Mit dem letzteren Werte wird $\lambda = 163°$, $\beta = 54°$, die Differenz gegen PTOLEMAIOS 13°, 0°.

No. 22 Aldiraan soll nach mehrfachen Angaben in den MS. « in fronte leonis » liegen, dann müsste die Altitudo LXXII statt LII sein. Dieselbe Position findet sich auch in jüngeren MS. z. B. in der tabelle zu CHAUCERS Arbeit vor (18), ebenso in Cod. Vatic. Palat. lat. 1381, saec. XV. Auch bei diesem Stern Wird die Worterklärung hinfällig.

No. 26 Arucuba müsste dem Namen nach ψ Ursae maj. sein. Dieser Stern aber hat bei PTOLEMAIOS $\lambda = 122°$, $\beta = 35°$. Hier ist demnach doch μ Ursae maj. anzunehmen.

No. 27 Egregez kommt zwar dem Namen nach auf dem Berliner Astrolab nicht vor, ist aber offenbar identisch mit jad al-dubb, ι Ursae maj. So stellen die beiden letzten Sterne zwei von den drei kafza (Sprünge) dar, wie ABDERRAHMAN SUFI die drei Paare Sterne in den Füssen des grossen Bären nennt. (18)

Die sehr grosse Abweichung bei No. 3 ist nicht zu erklären. Dass aber Alhaum wirklich α Serpentarii bedeutet, folgt daraus, dass der Stern mit gleichem Namen aber richtiger Positionsangabe in späteren MS. vorkommt, z. b. Cod. Vatic. Palat. Lat. 1398 fol. 14. In diesem Codex steht auch die erste Seite der Schrift Hermanns. Der Codex stammt aus dem 13. oder 14. Jahrh.

Im Salzburger Cod. Petr. a. V. 7 ist in der Sterntabelle durch den Schreiber eine Unordnung entstanden. Bei No. 20 steht Malenxe vel Algenxe, bei No. 21 Malerixe; dies ist aber ein dritter Name zu No. 20, und die daneben stehende Position gehört zum folgenden Stern Algomeisa. Eben so stehen die folgenden Positionen eine No. zu hoch, und es bleibt für den letzten Stern keine Position übrig. Der Schreiber setzte nun Egregez mit Arucuba in eine Zeile. Dieser Fehler ist auch in die Drucke von PEZ und MIGNE übergegangen.

(18) R. T. GUNTHER, Early science in Oxford, V. Photogr. 70v.

Wenn man nun berücksichtigt, dass HERMANN seine Zahlen nur in ganzen Graden angibt, ferner dass ein Fehler in θ bei höheren Declinationen den doppelt so grossen Fehler in δ bewirkt, so muss eine Abweichung von ± 1 Grad als erlaubt angesehen werden. Unter dieser Voraussetzung und der Annahme der obigen Korrekturen für Alhapit und Benenaz wird die Differenz gegen PTOLEMAIOS bei β achtzehnmal 0°, bei λ neunzehnmal 13°. Die Sterntabelle entspricht demnach der Zeit um 1000 n. Chr.

(Dorsten in Westfalen) J. DRECKER.

Salomon Maimons Philosophie der Mathematik[1]

SALOMON MAIMONS Beiträge zur Philosophie der Mathematik verdienen eine besondere Darstellung. Seine Ansichten über Mathematik sind in seinen Schriften zerstreut und an keiner Stelle finden wir eine zusammenhängende Gesamtdarstellung. Die Mathematik erfüllt aber in seinem System eine sehr bedeutsame Rolle. M. ist Rationalist und Skeptiker zugleich. In Bezug auf die Erfahrungswissenschaften nimmt er die skeptische Haltung HUMES ein. Wenn er sich dennoch mit vollem Rechte einen Rationalisten und Schüler LEIBNIZENS nennen kann, so geschieht das deswegen, weil ihm die Mathematik ein Vorbild der Wissenschaft ist, wie sie sein *könnte*, wenn unser Verstand dazu hinreichte. Er ist also nicht prinzipieller Skeptiker, sondern bezweifelt nur die Fähigkeit des menschlichen Geistes, die Rationalität der Erfahrung zu beweisen und damit sein eigenes Werk zu begründen. Aber die Mathematik zeigt uns, dass im Prinzip wenigstens Rationalität möglich ist, daher ihre grundlegende Bedeutung für das System der Philosophie.

Es steht zu erwarten, dass bei einem Denker, für welchen die Tatsache der Mathematik ein so entscheidendes Gewicht hatte, auch die Ausführungen, welche die Philosophie der Mathematik als solche betreffen, nicht ohne Interesse sein werden.

Von Bedeutung sind vor allem M's Ausführungen zur Theorie der Axiome. Man kann sagen, dass er wichtige Gedanken der modernen Mathematik vorweggenommen hat, indem er die

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(1) Abgekürzt zitierte Werke MAIMONS: Tr. = Versuch über die Transscendental-philosophie (1790), Ph. W. = Philosophisches Wörterbuch (1791), Str. = Streifereien im Gebiete der Philosophie (1793), V. = Versuch einer neuen Logik (1794), Kat. = Kategorien des ARISTOTELES (1794), K. U. = Kritische Untersuchungen (1797).

Gegenstände der Mathematik einzig und allein durch die *Verhältnisse* bestimmt sein lässt, in welche sie eintreten. Die Objekte der reinen Mathematik haben keine inneren Merkmale, sondern werden nur definiert durch die Verhältnisse, in welchen sie stehen. « Innere Merkmale sind solche, wodurch ein Gegenstand an sich, Verhältnisse aber solche, wodurch ein Gegenstand nicht an sich, sondern blos in Vergleichung mit einem anderen Gegenstande, bestimmt werden kann. Verhältnisse setzen immer innere Merkmale voraus, zwischen welchen diese Verhältnisse statt finden. Verhältnisse zwischen a priori, blos durch diese Verhältnisse gedachten Objekte aber setzen *keine inneren Merkmale*, wodurch die Objekte bestimmt werden, voraus, sondern sie bestimmen selbst die Objekte. Von der ersten Art sind z.B. die Objekte der angewandten, von der zweiten aber die der reinen Mathematik. Sollen z.B. zwei Körper, in Ansehung ihrer Grösse, als im Verhältnisse von zwei zu eines gegeben, gedacht werden, so müssen sie in Ansehung ihrer Qualität auf eine bestimmte Art gegeben sein, weil Grösse überhaupt ohne etwas das gross ist, nicht gegeben werden kann. Die Zahlen Eins, Zwei, an sich (von ihrer Anwendung abstrahiert) gedacht, sind a priori durch Verhältnis bestimmte Objekte der reinen Arithmetik. Diese haben keine andern inneren Merkmale, als die gedachten Verhältnisse selbst. » (V. 229) « Alle reinen Urteile sind Definitionen der Begriffe » heisst es Tr. 107.

Enthält also das Axiom nichts mehr als die Definition des Begriffs, so kann es keinen Anspruch auf Wahrheit machen. Diese Lehre findet sich bei M. wiederholt ausgesprochen. Die Prinzipien einer Wahrheit, sagt er, können nicht selbst Wahrheit sein. Wahr ist die Operation des Denkens, welche an sie ansetzt. « Demzufolge kann man nicht eigentlich sagen, ein mathematisches Axiom sei objektiv wahr, sondern bloss, es sei reell, d.h. es nützt zur Erkenntnis der Wahrheit und ihrem Gebrauche ». (Tr. 406, vgl. KRONER, *Von Kant bis Hegel* I, 345) « Logische Wahrheit ist die *Verknüpfung* der Objekte des Denkens (Begriffe), den Gesetzen des Verstandes gemäss. Die Axiomata sind die Elemente der Wahrheit, aber nicht Wahrheit selbst. Die aus der Verknüpfung herausgebrachten Resultate sind Produkte der Wahrheit, aber nicht Wahrheit selbst; weil, meiner Erklärung zufolge, Wahrheit bloss den Gang des Verstandes, oder seine gesetzmässige Art zu denken bedeutet, nicht aber das Prinzip, wovon er ausgegangen, auch nicht das

Resultat, wozu er zuletzt gelangt ist. Alle Sätze (auch die metaphysisch falschen) könnten als Prinzipium der logischen Wahrheit gebraucht werden, nicht nur deswegen, weil man aus falschen Prinzipien zufälliger Weise Wahrheiten herausbringen kann, sondern auch absolut, d.h. : unter Voraussetzung, dass diese falschen Sätze wahr sind, so muss dieses und dieses daraus folgen. Freilich werden diese Folgen sowohl als ihre Prinzipien von keinem praktischen Gebrauch sein, aber ich betrachte auch hier bloss ihren Gebrauch im Denken. Hätte EUKLIDES anstatt seiner metaphysisch wahren Axiomen falsche angenommen, so bin ich doch sicher, dass er nicht deswegen ein kleineres oder schlechteres Werk der Welt hinterlassen hätte, als dasjenige, was wir von ihm noch jetzt haben. Ich nehme z.B. an : dass der äussere Winkel eines Dreiecks nicht der Summe der beiden gegenüberstehenden inneren Winkel, sondern dieser Summe plus der Hälfte derselben gleich ist : so wird daraus notwendig folgen, dass der Winkel am Mittelpunkt des Zirkels nicht zweimal (wie er wirklich ist), sondern dreimal so gross ist, als der an der Peripherie, und dgl. Nehme ich an, ein Teil ist grösser als das Ganze, so würde ich daraus, den Gesetzen des Denkens gemäss, sowohl als aus dem gegengesetzten Axioma Folgen, die von jenen Folgen verschieden sind, herleiten. Zwar würde es der Richter nicht zugeben, dass ich nach dieser Voraussetzung meinem Gläubiger, dem ich einen Thaler schuldig bin, einen Groschen dafür bezahlen sollte, weil diesem zufolge ein Groschen noch mehr, als ein Thaler ist; dieses tut aber im Gebrauch des Verstandes nichts. Ich teile daher lieber die Sätze in reelle und nicht reelle, statt der wahren und falschen, ein; der Unterschied zwischen den reellen und nicht reellen (in Ansehung des Denkens) wird bloss darin bestehen, dass nämlich die letzteren zum wenigsten einen rellen Satz erfordern, ohne welchen sie auch im Denken keinen Gebrauch haben werden, nämlich den Satz des Widerspruchs. Diese Behauptung befördert nicht nur das Interesse der Vernunft, indem sie uns neue Aussichten zu ihrem Gebrauch eröffnet (dass wir z.B. eine neue Mathematik erfinden könnten), sondern sie hat auch zu ihrem Gebrauch in der Moral ihren Nutzen, dass wir nämlich in solchen Fällen, wo es keine wichtige Folge hat, unsern Eifer in Beibringung der Wahrheit und Benehmung des Irrtums in etwas mässigen sollen : denn es kann allerdings Falschheiten

geben, die einem gewissen Menschen viel nützlicher sind, als ihre entgegengesetzten Wahrheiten. » (Tr. 148 ff.)

Mit letzterer pragmatistischen Behauptung nähert sich M. NIETZSCHES Theorie von den nützlichen Irrtümern (in der « Fröhlichen Wissenschaft »), nur geht er nicht so weit wie Nietzsche, der diese nützlichen Irrtümer mit der Wahrheit identifiziert. Für uns ist M's Hinweis auf die Möglichkeit der Erfindung einer neuen Mathematik durch Zugrundelegung neuer Axiome von besonderer Wichtigkeit. Schon KUNTZE verwies in seinem Buche über M. (Seite 375) auf den Zusammenhang mit der nicht euklidischen Geometrie. M. ist hier offenbar ganz im Einklange mit der Auffassung des Axiomensystems als logischer Leerform möglicher Wissenschaften (« doctrinal function »), die wahr wird erst dadurch, dass für die Grundbegriffe eine solche Bedeutung aufgewiesen wird, zufolge deren die Axiome zu wahren Aussagen werden. (WEYL, *Philosophie der Mathematik*, S. 21). Dem Mathematiker selbst kommt es nur auf die Zusammenhänge an, nicht auf die Wahrheit der Axiome, von denen er ausgeht. Die mathematischen Sätze erheben daher keinen Anspruch auf objektive Wahrheit. Sie sind, sagt M., « objektiv wahr, nur unter Voraussetzung der Objektivität ihrer Grundsätze; sonst sind sie, wie die Grundsätze selbst, bloss subjektiv wahr ». (Tr. 405, Ph. W. 175).

Was WEYL Leerform möglicher Wissenschaften nennt, hat M. durch den ausgezeichneten Vergleich der Wahrheit mit einer Münze dargetan. (Tr. 406, 415). Eine Münze ist Münze im eigentlichen Verstande durch ihren Wert als *Zeichen;* dass sie selbst ausserdem als Ware einen Metallwert hat, ist für ihre Funktion als Münze, d.h. als Symbol, nur abträglich. Je weniger Wert sie als Ware hat, desto mehr Münze wird sie, weil sie von den Schwankungen des Warenwertes unabhängig wird. « Die idealische Münze hat also als Massstab des Wertes der Waren einen Vorzug vor der reellen, die selbst eigenen Wert als Ware hat. Aehnlich ist es mit der Wahrheit. Sie ist umso tauglicher für ihre Funktion, je mehr sie eine blosse Form oder Art, das Verhältnis der Dinge untereinander zu denken, ist, und als eine solche (blosse Leerform) bleibt sie unveränderlich, und ist hierin mit der bloss idealischen Münze zu vergleichen. Der Vergleich kann noch weiter geführt werden. Eine rein symbolische Münze

ist als Masstab vortrefflich. Sie erleichtert den Tausch, da ohne solchen Masstab die Menschen nur Waren direkt tauschen könnten, wenn jede der handelnden Personen die Ware des anderen bedurfte und die seinige missen konnte, sonst aber nicht. Aber eine solche symbolische Münze ist nur im inländischen, nicht aber im auswärtigen Handel zu gebrauchen. Aehnlich ist es mit der Wahrheit. « So lange man bloss bei der anschauenden Erkenntnis bleibt, geschieht die Erfindung der Wahrheit durch einen unmittelbaren Tausch, d.h. eine unmittelbare Substitution der Gedanken unter einander. Dieses hat zwar den Vorteil, dass man immer von der Realität der Gedanken sicher sein kann; hingegen hat es diese Unbequemlichkeit, dass man damit in Erfindung der Wahrheit, besonders wenn sie zu sehr versteckt ist, nicht weit kommen kann. Um dieser abzuhelfen, bedient man sich der symbolischen Erkenntnis, d.h. substituiert erstlich die Zeichen statt der bezeichnenden Dinge; zweitens substituiert man einem jeden Zeichen, ein ihm gleichgültiges Zeichen u.s.w., wodurch mit jeder neuen Formel eine neue Wahrheit entspringt. Dadurch ist man im Stande, ohne viele Mühe gleichsam mechanisch, die allerverborgensten Wahrheiten zu entdecken; es entspringt aber daher eine neue Schwierigkeit; nämlich, man gerät zuweilen auf symbolische Combinationen oder Formeln, die keine Realität haben, d.h. denen kein reeller Gegenstand entspricht, wie z.B. die imaginären Zahlen, Tangens, Cosinus eines rechten Winkels und dgl. in der Mathematik. Die symbolische Erkenntnis ist also zwar ein vortreffliches Hilfsmittel zur Erfindung der Wahrheit, dessen Gebrauch aber sehr viel Behutsamkeit erfordert; man muss bei jedem Schritte, so man darin tut, sich selbst nach der Sprache der Politiker, fragen : ob auch diese idealische Münze realisiert werden kann? Tut man dieses nicht, so gerät man auf die allerseltsamsten Ideen, aus denen man sich hernach nicht herauswickeln kann. Die Mathematik hat zwar durch ihre neuere Analysis viel gewonnen, indem man dadurch auf Entdeckungen geraten ist, die nach der Methode der Alten fast unmöglich waren; aber dadurch sind auch die unbehutsamen Mathematiker auf Schwierigkeiten geraten, wovon die Alten nichts wussten, wie aus den angeführten Beispielen erhellet. » (Tr. 411 ff.)

Wir würden dennoch fehl gehen, wollten wir M's Lehre so deuten, dass er rein formalistisch eingestellt ist und die Axiome

ür völlig willkürliche Annahmen hält. Sagt er doch selbst an der angeführten Stelle : « Hätte EUKLIDES anstatt seiner *metaphysisch wahren* Axiome falsche angenommen etc. »; er ist also überzeugt, dass die Axiome des EUKLIDES metaphysisch wahr sind. Aber sie sind nicht evident, sie sind bloss « subjektiv notwendig », (Ph. W. 175) und ihre objektive Notwendigkeit « kann daher mit Recht bezweifelt werden, vielleicht kann irgend ein denkendes Wesen oder auch ich selbst unter gewissen Umständen » andere Sätze zugrunde legen. Hierin würde kein Widerspruch liegen. Das Axiomensystem unserer Mathematik ist nicht das einzige, das widerspruchslos und möglich ist. Die Sätze sind subjektiv notwendig in dem Sinne, dass sie uns durch die Einbildungskraft *aufgedrungen* werden, ohne dass unser Verstand sie einsähe.

« Der Verstand schreibt dem produktiven Einbildungsvermögen eine Regel vor, einen Raum in drei Linien eingeschlossen, hervorzubringen; diese gehorcht und konstruiert das Dreieck, aber siehe, es dringen sich zugleich drei Winkel auf, die der Verstand gar nicht verlangt hatte. Nun wird er auf einmal klug, indem er die ihm bisher unbekannte Verknüpfung zwischen drei Seiten und drei Winkeln einsehen lernt, deren Grund aber ihm noch bis jetzt unbekannt ist. Er macht also aus der Not eine Tugend, indem er eine gebietrische Miene annimmt und sagt : Ein Dreieck muss drei Wihkel haben, als wäre er hierin selbst Gesetzgeber, da er doch in der Tat einem ihm gänzlich unbekannten Gesetzgeber gehorchen muss ». (Ph. W. 174). Wenn wir eine Erkenntnis als *rein* bezeichnen, die bloss ein Produkt des Verstandes (nicht der Sinnlichkeit) ist, so werden wir sagen müssen, (Tr. 56) dass die mathematischen Sätze wohl a priori sind (sie bedürfen keiner Bestätigung durch die Erfahrung), aber nicht rein; die Anschauung, aus der sie erfliessen, ist nicht von mir selbst nach einer Regel herausgebracht, sondern mir von irgend anderswo her, obschon a priori gegeben ». M. sagt daher von den Axiomen der Mathematik : « obschon sie materialiter in Zeit und Raum a priori sind, so sind sie formaliter es doch nicht » (Tr. 169). Wir folgen also hier einem apriorischen Zwang und nicht der Einsicht; während wir also von der implikativen Form der mathematischen Wahrheit mit Sicherheit sagen können, dass sie einen allgemeinen Wahrheitswert hat, hat die durch den *Inhalt* der Axiome bestimmte

« Materie hingegen nur bei uns, nicht aber bei jedem denkenden Wesen überhaupt, einen Wert » (Tr. 415).

KANT hatte auf die Frage, wie reine Mathematik als synthetische Erkenntnis a priori möglich sei, geantwortet (*Prolegomena* § 11), sie sei nur dadurch möglich, dass sie auf keine andere als blosse Gegenstände der Sinne geht, also nur auf unsere Erfahrung beschränkt ist. Diese Antwort kann M. annehmen. Denn da die mathematischen Axiome der « subjektiven Notwendigkeit » entspringen, welche unsere Anschauung beherrscht, so können wir sicher sein, dass *unsere* Erfahrung von den Axiomen beherrscht wird. Aber diese Rechtfertigung hat die entscheidende Lücke, dass sie nicht die *objektive* Notwendigkeit der Axiome aufweist. Es wird also bloss die Allgemeinheit a priori der mathematischen Urteile in Beziehung auf die ihnen zu subsumierenden Erfahrungsobjekte, als eine Folge ihrer Notwendigkeit, nicht aber diese Notwendigkeit selbst erklärt. « Ich denke dieses bestimmte Objekt auf diese bestimmte Art. Warum? Weil es in der *Konstruktion* nicht anders dargestellt wird » ; aber diese Konstruktion gibt nicht Begreiflichkeit. « Ein solcher Satz ist, solange seine Notwendigkeit nicht aus einem allgemeinen Grundsatz erklärbar ist, mit einem Satz a posteriori zu vergleichen ». (V. 414).

Wenn in den letzten Ausführungen die *Konstruktion*, welche uns die mathematischen Axiome liefert, als uneinsichtiger Zwang geschildert wird, dem wir uns fügen müssen, ohne seine objektive Notwendigkeit einzusehen, so ist andererseits zu sagen, dass diese « Konstruktion » oder « Darstellung » dennoch für uns eine Erkenntnis*quelle* ist. Dort wo die Begriffe uns nicht genug durchsichtig sind, um die Widerspruchslosigkeit eines Satzes zu beweisen, z.B. des Satzes « Die gerade Linie ist die kürzeste », springt die Konstruktion helfend ein: « Da dieser Satz in einer wirklichen Konstruktion gebracht wird, so erhellt hieraus, dass er keinen Widerspruch enthält. » (Tr. 358). Es ist das die heute von WEYL als « Modellmethode » bezeichnete Art, die Widerspruchslosigkeit nachzuweisen. Es wird ein Objekt *aufgewiesen*, das den betreffenden Satz realisiert. Freilich könnte auch die Anschauung selbst Widersprechendes realisieren und daher beschäftigt M., wie wir gleich sehen werden, die Frage, ob die Anschauung den Regeln des Verstandes gehorcht, aufs nachdrücklichste. Aber er traut der mathematischen Anschauung trotz ihrer mangelnden Einsichtigkeit. Während er

der physikalischen Erfahrung gegenüber die Frage aufwirft, ob ihre begriffliche Beherrschbarkeit nachgewiesen werden könne und diese Frage verneint, ist er bei der Mathematik nicht im Zweifel, dass ihre Materie verstandesdurchdrungen sei, weil diese Materie die a priorische (wenn auch nicht « reine ») Anschauung ist. Während z.B. nach WEYL eine « Konstruktion » an einem euklidischen Modell die Widerspruchslosigkeit eines Axiomensystems nur für denjenigen beweist, « der an die Wahrheit und damit an die Widerspruchslosigkeit der euklidischen Geometrie glaubt », während also auch dann, wenn wir ein Axiomensystem in der euklidischen Geometrie zu realisieren vermögen, noch immer der Zweifel bleibt, ob denn nicht doch noch die euklidische Geometrie einen Widerspruch enthält, ist für M. die Anschauung, das konkrete euklidische Modell, hinreichende Gewähr. Die Verstandesgemässheit — in seiner Sprache : Regelmässigkeit — der mathematischen Anschauung wird von ihm *nicht* in Zweifel gezogen. Im Gegensatz zur Anschauung, welche der *Physik* als Grundlage dient, und deren Verstandesgemässheit wir nicht nachweisen können, ist die *mathematische* Anschauung von der Einbildungskraft entsprechend den Regeln des Verstandes hervorgebracht worden. « Die Mathematik bestimmt ihre Gegenstände a priori durch Konstruktion; folglich bringt darin das Denkungsvermögen sowohl die *Form* als die *Materie* des Denkens aus sich selbst heraus. » (Tr. 2).

Zu diesem letzteren bemerkt KRONER (*Von Kant zu Hegel* I, 346) mit Recht : Seine Einschränkung findet dieses freie Produzieren der mathematischen Objekte aber darin, dass es nicht der reine Verstand ist, der sie erzeugt, sondern die produktive Einbildungskraft; dass wir daher in diesem Erzeugen doch nicht nur frei, sondern auch gebunden, nicht nur tätig, sondern auch leidend sind, — oder dass wir die Objekte zwar nach einer Regel entstehen lassen, dass es aber nicht der Verstand, die Regel selbst ist, welche sie zur Entstehung bringt ». Die Synthesis ist, wie M. sagt, « *regelmässig*, aber nicht *regelverständig* ». (Tr. 20). Dennoch aber liegt in dieser gleichzeitigen Erzeugung der Denkform und Denkmaterie in der Mathematik etwas, was uns Gott gleich macht. « Gott denkt nicht wie wir diskursiv, sondern seine Gedanken sind zugleich Darstellungen. Wird man einwenden, dass wir von einer solchen Denkart keinen Begriff haben, so antworte

ich; wir haben allerdings einen Begriff davon, indem wir dieselbe zum Teil selbst besitzen. Alle Begriffe der Mathematik werden von uns selbst gedacht, und zugleich als reelle Objekte durch Konstruktion a priori dargestellt. Wir sind also hierin Gott ähnlich. Kein Wunder also, dass die alten Philosophen die Mathematik hochgeschätzt haben ». (Tr. 20). « Der Verstand bringt die Objekte der Mathematik durch eine Konstruktion a priori, d.h. durch Verknüpfung seiner Form mit einer Anschauung a priori (dem reinen Raume) hervor. Beide sind zur Hervorbringung eines mathematischen Objekts notwendig. Die Form allein (der diskursive Begriff z.B. eines körperlichen regulären Dekaeders) ohne Konstruktion hat keine objektive Realität. Sie ist ein Objekt des Anschauungsvermögens, nicht aber des Verstandes oder Denkvermögens. Die Form und die Materie gehen also hier im Objekte nicht einander voraus. Der Verstand ist also nicht vermögend, alles zu denken, was keinen Widerspruch enthält, (so dass dadurch ein Objekt hervor gebracht werde) sondern nur dasjenige, wozu eine Anschauung a priori gegeben ist, d.h. was konstruiert werden kann. Er kann dem Begriff eines körperlichen regulären Dekaeders keine Realität geben, d.h. ein Objekt demselben gemäss hervorbringen. Die Materie (die Anschauung) widerstrebt ihm hierin ». (Str. 35).

M. übernimmt also hier von KANT den Unterschied von diskursivem und reellem Denken (das von einer Konstruktion oder anschaulichen Darstellung begleitet ist) und schreibt der « Darstellung » eine besondere Erkenntnisfunktion zu. Das von LEIBNIZ (2) übernommene Beispiel vom regelmässigen Dekaeder illustriert sie : Der Verstand findet hier keinen Widerspruch vor, die Darstellung aber zeigt die Grenzen dieser Verstandeserkenntnis. Sie « widerstrebt » dem Verstande. Dort also, wo unser Verstand nicht hinreicht, um über die « Regelverständigkeit » eines Gebildes zu entscheiden, kommt ihm die sozusagen klügere Anschauung zu Hilfe und entscheidet, wenn nicht über die Regelverständigkeit, so doch über die Regelmässigkeit des Gebildes, denn das anschaulich nicht Realisierbare muss auch begrifflich widersprechend sein, auch wenn unser Verstand diesen Widerspruch z.B. beim regelmässigen Dekaeder nicht einzusehen vermag.

(2) Nouv. essais III_3 § 15 « die Wesenheiten zu sein nur scheinen, aber es nicht sind, wie z. B. die eines regelmässigen Dekaeders ».

Die mathematische Erkenntnis ist also eine aus Verstandesform und Anschauungsstoff gemischte, wobei der Stoff nicht vom Erkenntnisvermögen hervorgebracht, sondern ihm a priori gegeben ist. (V. 122, 125). Wohl also ist auch in der Mathematik etwas « gegeben », aber dieses ist a priori gegeben, zum Unterschiede von der a posteriorischen Gegebenheit, auf welche die Naturwissenschaften angewiesen sind. Könnten wir *beweisen*, dass die anschauungsmässige Gegebenheit von Raum und Zeit, die der Mathematik zugrunde liegt, regelmässig ist, dass die Wirkung der Sinnlichkeit identisch ist mit der Wirkung des Verstandes (und uns nur nicht als solche durchsichtig), dann wäre freilich die metaphysische Wahrheit der mathematischen Axiome nachgewiesen. « Nur unter dieser Voraussetzung, dass nämlich die Wirkungen der Sinnlichkeit, Einbildung u.s.w. eben die Wirkung des Verstandes und der Vernunft, obgleich mit minderer Vollständigkeit ist, kann die Evidenz der Mathematik dargetan werden, sonst aber nicht ». (Tr. 348).

Diese Voraussetzung machen wir wohl, können sie aber nicht beweisen. Da die mathematischen Axiome für uns erst durch die Anschauung konstruierbar werden, sind sie synthetisch. Dies ist aber nicht ihre objektive Eigenschaft, vielmehr ist dies nur auf Rechnung unseres beschränkten Verstandes zu setzen und bei einem unendlichen Verstande müssten die Sätze analytisch sein. (Tr. 61, 181, K. U. 176). Wenn aber M. mit KANT darin übereinstimmt, dass die Axiome der Mathematik synthetisch sind, so weicht er darin von ihm ab, dass er ausser den mathematischen Sätzen für keine anderen synthetischen Urteile a priori die « Realität », d.h. den legitimen Erfahrungsgebrauch zugibt. Die obersten Sätze der Naturwissenschaft haben im Gegensatz zu den mathematischen Axiomen keinen legitimen Erfahrungsgebrauch, da sich die sinnliche Erfahrung nicht als Spezialfall der obersten Sätze der Naturwissenschaft nachweisen lässt. Zwischen der « reinen Naturwissenschaft » im Sinne KANTS und der Empirie besteht kein logischer Zusammenhang und der Erfahrungsgebrauch jener obersten Grundsätze lässt sich also nicht nachweisen. Dagegen besteht innerhalb der Mathematik ein Verhältnis der Ueber- und Unterordnung (Verhältnis der « Bestimmbarkeit »). So « bestimmen » (spezialisieren) die geometrischen Figuren den Raum wie die Species die Gattung « bestimmt. ». Der Raum

geht den geometrischen Figuren voraus, wie die Gattung de: Spezies, das Bestimmbare dem Bestimmten. « Die Objekte der Mathematik setzen also die reinen Begriffe von Bestimmbaren und Bestimmung, und die Grundsätze der Bestimmbarkeit, die sich auf die Objekte der Mathematik absolut a priori (vor der bestimmten Erkenntnis derselben) beziehen, voraus ». (V. 327). « Darauf beruht die Notwendigkeit und Allgemeingültigkeit der Mathematik : denn da das absolute Bestimmbare in den Objekten der Mathematik, (Raum oder Zeit) dem Erkenntnisvermögen a priori gegeben, und Bestimmung ohne das Bestimmbare unmöglich ist, so ist die Verbindung beider in einer Einheit des Bewusstseins notwendig und folglich auch allgemeingültig ». (V. 125).

Ein jedes Objekt der Mathematik besteht aus dem Stoff (Raum und Zeit) und einer besonderen Form, welche beide a priori sind. Der Stoff wird nicht vom Erkenntnisvermögen « regelverständig » hervorgebracht, sondern der Erkenntnis a priori von der « regelmässig » d.h. dem Erkenntnisvermögen entsprechend wirkenden Einbildungskraft gegeben. Aus diesem Stoff werden die mathematischen Gegenstände nach dem Grundsatz der Bestimmbarkeit vom Erkenntnisvermögen hervorgebracht. Während unsere tägliche Erfahrung sich nicht als Spezialfall der a priori konstruierten reinen Naturwissenschaft nachweisen lässt und daher der Zweifel HUMES der Erfahrung gegenüber gerechtfertigt ist, sind die *mathematischen* Bestimmungen unserer Erfahrung als logische Besonderungen von Raum und Zeit (nach dem Satze der Bestimmbarkeit) zu verstehen und daher die Sätze auch der angewandten Mathematik synthetisch a priori. Daher ist es erkenntnistheoretisch etwas ganz anderes, wenn ich einem Ding die Kausalität zuspreche oder aber ihm die Rundheit zuspreche. M. illustriert das durch das immer wiederkehrende Beispiel vom Teller, dessen Rundung eine Besonderung des Raumes ist, während er sich nicht als Besonderung der Kausalität nachweisen lässt. Wenn ich urteile, dieser Teller ist rund, so vergleiche ich den Teller mit der a priori gedachten Regel, wodurch die runde Figur als Objekt entsteht und durch diese Vergleichung ein Merkmal des Tellers wird. Dahingegen ist das Verhältnis der Kausalität kein Merkmal der empirischen Objekte, auf die es bezogen wird.

Hierin haben wir also den Grund dafür zu suchen, dass der Skeptiker M. wohl die Physik, nicht aber die Mathematik in

Frage stellte. Die mathematischen Gebilde sind aus den reinen Anschauungen des Raumes und der Zeit nach dem Gesetze der Bestimmbarkeit *erzeugt*. Diese Erzeugung ist wohl nicht in allen ihren Teilen durchsichtig, denn wenn ich z.B. den Raum zu einem Dreieck bestimme, fügt sich mir ohne mein Zutun, durch die Konstruktion allein, die Tatsache hinzu, dass das Dreiseit drei Winkel hat. Aber wenn auch das mathematische Gebilde sofern nicht ganz durchsichtig ist, so ist es doch als Bestimmung und Besonderung a priorischer Formen fassbar. Dagegen ist ein physikalisches Ereignis, z.B. dass die Sonne den Stein erwärmt, in keiner Weise als Besonderung reiner Begriffe oder reiner Sätze zu verstehen, die Tatsachenwahrheiten der Physik sind in keiner Weise Besonderungen der ewigen Wahrheiten. Dieser Abgrund zwischen zwei Arten von Wahrheiten ist die Quelle der Skepsis M's. Sie hat aber in der Mathematik keinen Platz, weil dort das Verhältnis der Bestimmbarkeit herrscht, das wir ausserhalb der Mathematik nicht mehr vorfinden. Hierin liegt die besondere Bedeutung der Mathematik für die Erkenntnistheorie M's, indem sie die einzige Wissenschaft ist, in welcher man synthetische Urteile a priori findet (Kat. 133) und zwar kommen diese synthetischen Urteile a priori durch reine Konstruktion zustande, d.h. wir müssen die durch diese Urteile bestimmten Prädikate den Subjekten beilegen, weil sie nicht anders, wie mit diesen Prädikaten dargestellt werden können. (V. 327, 413). In der Physik dagegen ist die Anschauung nicht a priori, sondern a posteriori (Tr. 49). Die Objekte gehen der Wissenschaft voran und werden ihr zur Bearbeitung von der Erfahrung dargereicht. Dagegen ist die Mathematik die einzige Wissenschaft, wo die Objekte der Wissenschaft nicht vorhergehen, « weil sie selbst erst durch Wissenschaft als reelle Objekte konstruiert werden. » (Kat. 43).

Wir haben also M's Auffassung der Axiome als eine dualistische anzusehen. Solange wir auf dem Standpunkte des *Verstandes* allein stehen, haben die Axiome keine Wahrheit, wahr ist nur die Verknüpfung der Sätze untereinander. Ziehen wir aber neben dem Verstande eine zweite Erkenntnisquelle, die *Anschauung*, heran, so müssen wir zugeben, dass sie uns durch die « Konstruktion » oder « Darstellung » die Wahrheit der Axiome aufdrängt, ohne sie uns freilich durchsichtig zu machen. Die Axiome sind daher jetzt für uns synthetische durch die Anschauung allein

gewährleistete Wahrheiten, die zunächst im Vertrauen auf die Anschauung als subjektive und für Subjekte allgemeingültige Wahrheiten angenommen werden. Erst unter der Voraussetzung, dass die produktive Einbildungskraft, die Schöpferin der Anschauung, « regelmässig » (wenn auch nicht regelverständig) verfährt, könnten diese subjektiven Wahrheiten als objektive Wahrheiten angesehen werden, die vor dem unendlichen Verstande sich als analytische Wahrheiten herausstellen würden. Allein diese Voraussetzung wird von M. wohl zugrunde gelegt, aber nicht bewiesen.

Wir haben also hier die folgende Stufenfolge der Wahrheit der Axiome :

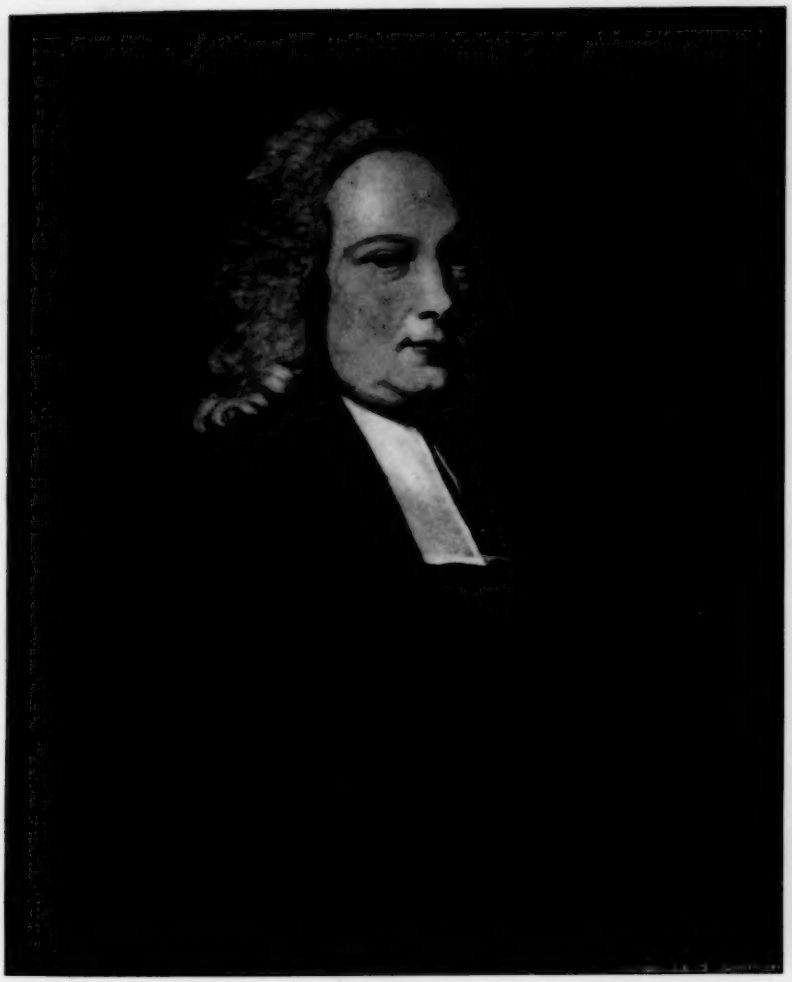
a) vom Gesichtspunkte des Verstandes können sie bloss als Definitionen betrachtet werden,

b) durch die Anschauung (« Konstruktion ») wird ihre « subjektive Wahrheit » gerechtfertigt. Diese Rechtfertigung erstreckt sich nicht nur auf die reine Mathematik, sondern auch auf die angewandte, da

c) die mathematischen Eigenschaften der Erfahrung (im Gegensatz zu den physikalischen) in einem logischen Abhängigkeitsverhältnis (« Verhältnis der Bestimmbarkeit ») zu den reinen Anschauungen von Raum und Zeit stehen,

d) bleibt immer die Möglichkeit offen, dass auch eine andere Anschauung als die unsere möglich ist und damit andere Axiome. Dass sich z.B. die Gattung Raum gerade in der Richtung « dreidimensionaler Raum » spezialisiert, ist zufällig. Sie könnte sich auch anders spezialisieren, gerade so wie die Gattung « Kegelschnitt » sich als Kreis, Parabel etc. spezialisieren kann. Für die logische Würde der Mathematik als eines Inbegriffs synthetischer Urteile a priori ist *dies* von entscheidender Bedeutung, dass eine logische Spezialisierung von den obersten Gattungen, Raum und Zeit, bis zu den konkreten mathematischen Gebilden der Erfahrung vorliegt. Es ist gerade dies das Moment, welches der Physik fehlt.

Jerusalem. HUGO BERGMANN.



JAMES BRADLEY
(1693-1762)
(Courtesy of National Portrait Gallery)

Discovery of the aberration of light

With facsimile reproduction (no. XII) *of* JAMES BRADLEY's *letter to* EDMOND HALLEY *giving an account of his discovery (Philosophical Transactions, no.* 406, *vol.* 35, 637-61, 1729).

1.—The aberration of light, that is, the apparent displacement of a heavenly body due to the combination of the orbital velocity of the earth with the velocity of light, was discovered by JAMES BRADLEY and the letter to EDMOND HALLEY in which he announced his discovery was read before the Royal Society on the 9th and 16th of January, 1729. One of the MSS. copies of it still extant is dated Jan. 1, 1728-29. It is well to insist upon the date 1729, because the letter is printed in the *Philosophical Transactions* for the month of December 1728. Hence the discovery is often dated 1728. The correct date is 1729.

2.—JAMES BRADLEY was born in Sherbourn, Gloucestershire, 1693, and educated at Balliol College, Oxford. His main instruction and training he received from his uncle, the Rev. JAMES POUND (1669-1724), rector of Wanstead in Essex, who was then the best astronomical observer in England. He began to work with his uncle about 1715, and was nursed by him when afflicted with smallpox two years later. Toward the end of that same year, 1717, EDMOND HALLEY introduced him to the scientific world and was the first to praise him. In 1721, BRADLEY succeeded JOHN KEILL (1671-1721) in the Savilian chair of astronomy of Oxford; in 1729, he succeeded J. WHITESIDE as lecturer in experimental philosophy and he continued in that lectureship until 1760. When HALLEY died in 1742, BRADLEY was appointed astronomer royal in his stead and went to live in Greenwich. Yet he spent three months of each year in Oxford to deliver his lectures.

3.—Under his uncle's guidance BRADLEY learned to observe

with the utmost accuracy then attainable. His first larger undertaking was a study of the Jovian system; he completed in 1718 « Corrected tables » relative to it, but these remained unpublished until 1749. A more fundamental problem then attracted his attention. If the Copernican theory was correct it ought to be possible to observe an annual parallax of the stars. In fact within the last three decades of the seventeenth century a number of striking observations had revived the interest in parallaxes. At the time of his voyage to Uraniborg (begun in 1671) JEAN PICARD noticed annual variations in the position of the polar star extending to nearly 40", but—and this is very remarkable—after having studied them he concluded that they could not be explained either by refraction or by parallax (1). A few years later, in 1674, ROBERT HOOKE made similar observations and lacking PICARD's prudence and method, he thought they were parallactic effects. FLAMSTEED made many observations between 1689 and 1697, and explained them in the same way as HOOKE. (2) However in 1699, J. CASSINI proved that the parallax would produce very different effects. A similar demonstration was given by E. MANFREDI, but neither of them suggested the true explanation. (3)

4.—In 1725, SAMUEL MOLYNEUX (1689-1728), conceived the idea of reviewing the whole problem. A special instrument was built for him by GEORGE GRAHAM for this very purpose and established in Kew, near London. MOLYNEUX made many observations in December of that year, apparently with a view of confirming the parallactic hypothesis. It is here that BRADLEY appears upon the scene. His own paper will tell the rest of the story. Suffice it to say that he disproved the parallactic explanation and suggested a new one,—the correct one—aberration of light.

5.—BRADLEY's success was due not only to his excellent instru-

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(1) J. B. J. DELAMBRE : Histoire de l'astronomie moderne (vol. 2, 616, Paris, 1821).

(2) The displacements observed by PICARD, HOOKE and FLAMSTEED were of course due to aberration but these astronomers were not aware of it, and as far as I know, the very conception of aberration never occurred to anybody before BRADLEY.

(3) DELAMBRE : Histoire de l'astronomie au XVIII^{e} siècle (Paris, 1827, 413).

mental means, to his own perfect experimental technique, but as well to his thoroughness and persistence. In that he was almost the opposite of HOOKE, the poor and ubiquitous HOOKE, who took part in almost every scientific controversy of his time but hardly ever succeeded in achieving anything of great importance because he did not carry his investigations deeply enough and never reached the bedrock of any problem. BRADLEY is one of the best examples of the « classical » type of scientists as opposed to the « romantic » type. (4) His thoughts were deep rather than brilliant and they matured but very slowly; he was anxious to improve his observations to the limit of his experimental possibilities and he succeeded in doing so; moreover he was all the time trying to improve the instruments themselves and to detect and measure their errors. Being inhibited by an extraordinary fear of error he published very little. However two of his very few papers are of great importance, for each of them contains the publication of a fundamental discovery. The first, explaining aberration (1729), is reprinted in facsimile below; the second, explaining nutation (1748), will be reproduced in an ulterior number of *Isis*. These two papers are truly foundation stones of modern astronomy.

6.—With regard to the aberration, I would like to remark that BRADLEY did not simply discover it but that his determination of it was, considering his instrumental means, extremely accurate. He concluded that the maximum aberration was included between 40" or 41" (see p. 655); the value of the constant of aberration accepted to-day is 20" 47 (that is 40" 94 for the whole axis). He deduced from this value the speed of light, and found that the sunlight would reach us in 8 m. 13 sec. (our present estimate is 8 m. 19 sec.).

7.—It is curious that the word aberration does not occur in BRADLEY's paper of 1729 in which he announced his discovery of it; but he uses it—with no suggestion of novelty—in his paper on nutation (1748). When was the word introduced and by whom, where, and how? It is possible that it was never formally defined.

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(4) A very good classification suggested by WILHELM OSTWALD : Grosse Männer (Leipzig, 1910, 44 and passim).

Miss C. Doris Hellman has kindly investigated this question for me. She has examined volumes 35 to 45 of the *Philosophical Transactions* for the years 1728 to 1748 and the volumes of the *Histoire de l'Académie royale des sciences* relative to the same period. The earliest use of the word aberration she found in a memoir by Clairaut « De l'aberration apparente des étoiles causée par le mouvement progressif de la lumière » in the second collection (année 1737, p. 205-226, Paris, 1740), memoir read on Dec. 11, 1737. The word aberration is not defined in it, Clairaut simply says (this being his first sentence) : « C'est à M. Bradley que l'Astronomie est redevable de la belle découverte de l'Aberration de la Lumière ».

The word aberration also occurs in a letter written by Maupertuis to Bradley in Paris, Sept. 27, 1737 : « according to your theory of the aberration occasioned by the motion of light ». (5) The phrase suggests that the word had not yet been formally defined. Hence we may perhaps conclude that the word was informally introduced in (or before) 1737.

8.—Either of these discoveries would have been sufficient to establish his fame, but if he had made only one—especially the first one—one might have ascribed part of his success to luck. The fact that he made both, and above all his manner of making them, proves that his main luck was to have been endowed with genius and with those moral qualities which sustain a man's attention toward a definite goal throughout a lifetime. And even if he had made neither, he would still have been remembered as one of the very best experimentalists of his time, and one of the founders of modern astronomical observation. He accumulated scientific data whose quality is as remarkable as their quantity. Their publication and reduction occupied many mathematicians for a long period after his death. Some 60,000 observations « Astronomical observations made at the Royal Observatory at Greenwich, from the year 1750 to the year 1762 » were published in two volumes by Thomas Hornsby (1733-1810), in 1798, and Abraham Robertson (1751-1826), in 1805. More than a thousand

(5) S. P. Rigaud : Miscellaneous works of J. Bradley (Oxford, 1832, p. 404). Quoted in English, the translation being apparently Bradley's own. The original French letter is in the archives of the Royal Society.

of his stars were included in GIUSEPPE PIAZZI's second catalogue (1814). Finally the observations relative to 3222 stars were reduced with the utmost care by FRIEDRICH WILHELM BESSEL and published by him under the significant title—Fundamenta astronomiae pro MDCCLV deducta ex observationibus JAMES BRADLEY in specula astronomica Grenovicensi per 1750-1762 institutis (Königsberg, 1818). BESSEL's work was fundamental indeed. Thanks to his zeal, —comparable only to BRADLEY's in another sphere—the astronomers were given an immense body of remarkably accurate data for the year 1755, and modern astronomy was made as it were half a century older. Even BRADLEY's earlier observations were thought worthy of publication or republication, and this was done by A. L. BUSCH : Reduction of the observations made by BRADLEY at Kew and Wansted to determine the quantities of aberration and nutation (Oxford, 1838).

9.—Among this mass of observations, I would select two sets, not because of their excellence, but because of their historical interest. They establish additional links between BRADLEY and EDMOND HALLEY, who was his predecessor in Greenwich and his scientific godfather. Toward the end of his life, in 1759, BRADLEY made many observations of HALLEY's comet—but these are unfortunately lost, except for a fragment. In 1761, he drafted instructions for the observations of the transit of Venus which were made by NEVIL MASKELYNE at St. Helena. The return of HALLEY's comet—watched for by all the astronomers of Europe—was a beautiful confirmation of HALLEY's theory, and the same had recommended the observation of the transit of Venus in order to determine the sun's parallax, as early as 1716.

10.—There is still one incident of BRADLEY's quiet existence which I would like to recall. The Gregorian reform of the calendar had failed to be accepted in England because of its « Popish » origin. By the middle of the eighteenth century the gap between the Julian and the Gregorian calendar was already considerable (11 days), and the ambiguities of English and continental dates were extremely annoying. The Earl of CHESTERFIELD (PHILIP DORMER STANHOPE, 1694-1773) introduced a bill for the adoption of the Gregorian reform, which after much opposition was finally carried in 1751. The tables appended to the bill had been calculated by BRADLEY, who shared the impopularity

of those who had caused the alteration of style. We hear an echo of those quarrels in one of VOLTAIRE's sallies.

« La populace anglaise aimait mieux voir le calendrier en désaccord avec le soleil que d'être d'accord avec le pape, et refusait d'accepter une réforme dont il aurait fallu savoir gré au grand Turc s'il l'avait proposée. » (6)

It is possible that BRADLEY's aloofness and reticence were aggravated by the sudden enmity of which he found himself the victim, and which followed him almost to the tomb. According to RIGAUD (7) « When BRADLEY, worn down by his labours in the cause of science, was sinking under the disease which closed his mortal carrer, many of the common people attributed his sufferings to a judgment from heaven for his having been instrumental in what they considered to be so impious an undertaking. »

11.—However, the enmity of some politicians and of their followers was largely compensated by the esteem and admiration of the learned world. He had been declared by NEWTON to be the best astronomer in Europe. In 1748, he received the Copley medal for his discovery of the nutation, and from that time on until the time of his death scarcely a year passed without his receiving some mark of recognition or gratitude either from his own countrymen or from foreign individuals or academies.

After many years of discomfort he died of a chronic inflammation of the abdominal viscera at Chalford, Gloucestershire, on July 13, 1762. He was buried near his mother and his wife at Minchinhampton.

Bibliographical Note

12. An elaborate account of BRADLEY's life and works was written by STEPHEN PETER RIGAUD (1774-1839), of the Radcliffe Observatory, and prefixed by him to his edition of the « Miscellaneous works and correspondence of the Rev. JAMES BRADLEY,

(6) I found this quotation in E. CAVAIGNAC, Chronologie (Paris, 1925, p. 11), who calls it « La boutade connue de VOLTAIRE ». However I failed to identify it in BEUCHOT's edition of VOLTAIRE's works. The nearest approach to it is in vol. 18, p. 356, but that is not half as good. I leave it as an example of how sallies are gradually improved and amplified,—and after all, VOLTAIRE may have written that somewhere.

(7) Memoirs of BRADLEY (p. LXXXI).

etc. » (CVIII + 528 p., 6 pl., portrait, Oxford, University Press, 1832).

13. There are many portraits of his, for which see the Memoirs (p. CIV, note m). The one which we are publishing with formal license to do so, is in the National Portrait Gallery, a reduced copy of the painting by THOMAS HUDSON in the picture gallery at Oxford.

Cambridge, Mass.
October 3, 1930. GEORGE SARTON.

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The following facsimile is reproduced thanks to the courtesy of the Library of Congress, of Mr. FREDERICK E. BRASCH, and of the Harvard Library (for p. 637, 658).

IV. *A Letter from the Reverend Mr.* James Bradley *Savilian Profeſſor of Aſtronomy at* Oxford, *and F.R.S. to Dr.* Edmond Halley Aſtronom. Reg. &c. *giving an Account of a new diſcovered Motion of the* Fix'd Stars.

SIR,

YOU having been pleaſed to expreſs your Satisfaction with what I had an Opportunity ſometime ago, of telling you in Converſation, concerning ſome Obſervations, that were making by our late worthy and ingenious Friend, the honourable *Samuel Molyneux* Eſquire, and which have ſince been continued and repeated by my ſelf, in order to determine the *Parallax* of the *fixt Stars*; I ſhall now beg leave to lay before you a more particular Account of them.

Before I proceed to give you the Hiſtory of the Obſervations themſelves, it may be proper to let you know, that they were at firſt begun in hopes of verifying and confirming thoſe, that Dr. *Hook* formerly communicated to the publick, which ſeemed to be attended with Circumſtances that promiſed greater Exactneſs in them, than could be expected in any other, that had been made and publiſhed on the ſame Account. And as his Attempt was what principally gave Riſe to this, ſo his Method in making the Obſervations was in ſome Mea-

Meaſure that which Mr. *Molyneux* followed: For he made Choice of the ſame Star, and his Inſtrument was conſtructed upon almoſt the ſame Principles. But if it had not greatly exceeded the Doctor's in Exactneſs, we might yet have remained in great Uncertainty as to the *Parallax* of the *fixt Stars*; as you will perceive upon the Compariſon of the two Experiments.

This indeed was chiefly owing to our curious Member, Mr. *George Graham*, to whom the Lovers of Aſtronomy are alſo not a little indebted for ſeveral other exact and well-contrived Inſtrumeuts. The Neceſſity of ſuch will ſcarce be diſputed by thoſe that have had any Experience in making Aſtronomical Obſervations; and the Inconſiſtency, which is to be met with among different Authors in their Attempts to determine ſmall Angles, particularly the annual Parallax of the *fixt Stars*, may be a ſufficient Proof of it to others. Their Diſagreement indeed in this Article, is not now ſo much to be wondered at, ſince I doubt not, but it will appear very probable, that the Inſtruments commonly made uſe of by them, were liable to greater Errors than many times that Parallax will amount to.

The Succeſs then of this Experiment evidently depending very much on the Accurateneſs of the Inſtrument that was principally to be taken Care of: In what Manner this was done, is not my preſent Purpoſe to tell you; but if from the Reſult of the Obſervations which I now ſend you, it ſhall be judged neceſſary to communicate to the Curious the Manner of making them, I may hereafter perhaps give them a particular Deſcription, not only of Mr. *Molyneux*'s Inſtrument, but alſo of my own, which

which hath ſince been erected for the ſame Purpoſe and upon the like Principles, though it is ſomewhat different in its Conſtruction, for a Reaſon you will meet with preſently.

Mr. *Molyneux's Apparatus* was compleated and fitted for obſerving about the End of *November* 1725, and on the third Day of *December* following, the bright Star in the Head of *Draco* (marked γ by *Bayer*) was for the firſt Time obſerved, as it paſſed near the Zenith, and its Situation carefully taken with the Inſtrument. The like Obſervations were made on the 5th, 11th, and 12th Days of the ſame Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Seaſon ſeemed needleſs, it being a Part of the Year, wherein no ſenſible Alteration of Parallax in this Star could ſoon be expected. It was chiefly therefore Curioſity that tempted me (being then at *Kew*, where the Inſtrument was fixed) to prepare for obſerving the Star on *December* 17th, when having adjuſted the Inſtrument as uſual, I perceived that it paſſed a little more Southerly this Day than when it was obſerved before. Not ſuſpecting any other Cauſe of this Appearance, we firſt concluded, that it was owing to the Uncertainty of the Obſervations, and that either this or the foregoing were not ſo exact as we had before ſuppoſed; for which Reaſon we purpoſed to repeat the Obſervation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star paſſed ſtill more Southerly than in the former Obſervations. This ſenſible Alteration the more ſurprized us, in that it was the contrary way

way from what it would have been, had it proceeded from an annual *Parallax* of the *Star*: But being now pretty well ſatisfied, that it could not be entirely owing to the want of Exactneſs in the Obſervations; and having no Notion of any thing elſe, that could cauſe ſuch an apparent Motion as this in the Star; we began to think that ſome Change in the Materials, *&c.* of the Inſtrument itſelf, might have occaſioned it. Under theſe Apprehenſions we remained ſome time, but being at length fully convinced, by ſeveral Trials, of the great Exactneſs of the Inſtrument, and finding by the gradual Increaſe of the Stars Diſtance from the Pole, that there muſt be ſome regular Cauſe that produced it; we took care to examine nicely, at the Time of each Obſervation, how much it was: and about the Beginning of *March* 1726, the Star was found to be 20″ more Southerly than at the Time of the firſt Obſervation. It now indeed ſeemed to have arrived at its utmoſt Limit Southward, becauſe in ſeveral Trials made about this Time, no ſenſible Difference was obſerved in its Situation. By the Middle of *April* it appeared to be returning back again towards the North; and about the Beginning of *June*, it paſſed at the ſame Diſtance from the Zenith as it had done in *December*, when it was firſt obſerved.

From the quick Alteration of this Star's Declination about this Time (it increaſing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had gone Southward of its preſent Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became ſtationary,

tionary, being then near 20″ more Northerly than in *June*, and no leſs than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the ſame Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Preceſſion of the Equinox.

This was a ſufficient Proof, that the Inſtrument had not been the Cauſe of this apparent Motion of the Star, and to find one adequate to ſuch an Effect ſeemed a Difficulty. A Nutation of the Earth's Axis was one of the firſt things that offered itſelf upon this Occaſion, but it was ſoon found to be inſufficient; for though it might have accounted for the change of Declination in γ *Draconis* yet it would not at the ſame time agree with the Phænomena in other Stars; particularly in a ſmall one almoſt oppoſite in right Aſcenſion to γ *Draconis*, at about the ſame Diſtance from the North Pole of the Equator: For, though this Star ſeemed to move the ſame way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the Obſervations of both made upon the ſame Days, at different Seaſons of the Year) this plainly proved, that the apparent Motion of the Stars was not occaſioned by a real Nutation, ſince if that had been the Cauſe, the Alteration in both Stars would have been near equal.

The great Regularity of the Obſervations left no room to doubt, but that there was ſome regular Cauſe that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the

Seaſons of the Year. Upon comparing the Obſervations with each other, it was diſcovered, that in both the fore-mentioned Stars, the apparent Difference of Declination from the *Maxima*, was always nearly proportional to the verſed Sine of the Sun's Diſtance from the Equinoctial Points. This was an Inducement to think, that the Cauſe, whatever it was, had ſome Relation to the Sun's Situation with reſpect to thoſe Points. But not being able to frame any Hypotheſis at that Time, ſufficient to ſolve all the Phænomena, and being very deſirous to ſearch a little farther into this Matter; I began to think of erecting an Inſtrument for my ſelf at *Wanſted*, that having it always at Hand, I might with the more Eaſe and Certainty, enquire into the Laws of this new Motion. The Conſideration likewiſe of being able by another Inſtrument, to confirm the Truth of the Obſervations hitherto made with Mr. *Molyneux*'s, was no ſmall Inducement to me; but the Chief of all was, the Opportunity I ſhould thereby have of trying, in what Manner other Stars were affected by the ſame Cauſe, whatever it was. For Mr. *Molyneux*'s Inſtrument being originally deſigned for obſerving γ *Draconis* (in order, as I ſaid before, to try whether it had any ſenſible Parallax) was ſo contrived, as to be capable of but little Alteration in its Direction, not above ſeven or eight Minutes of a Degree: and there being few Stars within half that Diſtance from the Zenith of *Kew*, bright enough to be well obſerved, he could not, with his Inſtrument, throughly examine how this Cauſe affected Stars differently ſituated with reſpect

reſpect to the equinoctial and ſolſtitial Points of the Ecliptick.

Theſe Conſiderations determined me; and by the Contrivance and Direction of the ſame ingenious Perſon, Mr. *Graham*, my Inſtrument was fixed up *Auguſt* 19, 1727. As I had no convenient Place where I could make uſe of ſo long a Teleſcope as Mr. *Molyneux*'s, I contented my ſelf with one of but little more than half the Length of his (*viz.* of about 12½ Feet, his being 24½) judging from the Experience which I had already had, that this Radius would be long enough to adjuſt the Inſtrument to a ſufficient Degree of Exactneſs, and I have had no Reaſon ſince to change my Opinion: for from all the Trials I have yet made, I am very well ſatisfied, that when it is carefully rectified, its Situation may be ſecurely depended upon to half a Second As the Place where my Inſtrument was to be hung, in ſome Meaſure determined its Radius, ſo did it alſo the Length of the Arch, or Limb, on which the Diviſions were made to adjuſt it: For the Arch could not conveniently be extended farther, than to reach to about 61° on each Side my Zenith. This indeed was ſufficient, ſince it gave me an Opportunity of making Choice of ſeveral Stars, very different both in Magnitude and Situation; there being more than two hundred inſerted in the *Britiſh* Catalogue, that may be obſerved with it. I needed not to have extended the Limb ſo far, but that I was willing to take in *Capella*, the only Star of the firſt Magnitude that comes ſo near my Zenith.

My Inſtrument being fixed, I immediately began to obſerve ſuch Stars as I judged moſt proper to

give me light into the Cauſe of the Motion already mentioned. There was Variety enough of ſmall ones; and not leſs than twelve, that I could obſerve through all the Seaſons of the Year; they being bright enough to be ſeen in the Day-time, when neareſt the Sun. I had not been long obſerving, before I perceived, that the Notion we had before entertained of the Stars being fartheſt North and South, when the Sun was about the Equinoxes, was only true of thoſe that were near the ſolſtitial Colure: And after I had continued my Obſervations a few Months, I diſcovered, what I then apprehended to be a general Law, obſerved by all the Stars, *viz.* That each of them became ſtationary, or was fartheſt North or South, when they paſſed over my Zenith at ſix of the Clock, either in the Morning or Evening. I perceived likewiſe, that whatever Situation the Stars were in with reſpect to the cardinal Points of the Ecliptick, the apparent Motion of every one tended the ſame Way, when they paſſed my Inſtrument about the ſame Hour of the Day or Night; for they all moved Southward, while they paſſed in the Day, and Northward in the Night; ſo that each was fartheſt North, when it came about Six of the Clock in the Evening, and fartheſt South, when it came about Six in the Morning.

Though I have ſince diſcovered, that the *Maxima* in moſt of theſe Stars do not happen exactly when they come to my Inſtrument at thoſe Hours, yet not being able at that time to prove the contrary, and ſuppoſing that they did, I endeavoured to find out what Proportion the greateſt Alterations of Declination in different Stars bore to each other; it being very

very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneaux*'s Obſervations, that γ *Draconis* altered its Declination about twice as much as the fore-mentioned ſmall Star almoſt oppoſite to it; but examining the matter more particularly, I found that the greateſt Alteration of Declination in theſe Stars, was as the Sine of the Latitude of each reſpectively. This made me ſuſpect that there might be the like Proportion between the *Maxima* of other Stars; but finding, that the Obſervations of ſome of them would not perfectly correſpond with ſuch an Hypotheſis, and not knowing, whether the ſmall Difference I met with, might not be owing to the Uncertainty and Error of the Obſervations, I deferred the farther Examination into the Truth of this Hypotheſis, till I ſhould be furniſhed with a Series of Obſervations made in all Parts of the Year; which might enable me, not only to determine what Errors the Obſervations are liable to, or how far they may ſafely be depended upon; but alſo to judge, whether there had been any ſenſible Change in the Parts of the Inſtrument itſelf.

Upon theſe Conſiderations, I laid aſide all Thoughts at that Time about the Cauſe of the fore-mentioned Phænomena, hoping that I ſhould the eaſier diſcover it, when I was better provided with proper Means to determine more preciſely what they were.

When the Year was compleated, I began to examine and compare my Obſervations, and having pretty well ſatisfied my ſelf as to the general Laws of the *Phænomena*, I then endeavoured to find out the Cauſe

Cauſe of them. I was already convinced, that the apparent Motion of the Stars was not owing to a Nutation of the Earth's Axis. The next Thing that offered itſelf, was an Alteration in the Direction of the Plumb-line, with which the Inſtrument was conſtantly rectified; but this upon Trial proved inſufficient. Then I conſidered what Refraction might do, but here alſo nothing ſatisfactory occurred. At laſt I conjectured, that all the *Phænomena* hitherto mentioned, proceeded from the progreſſive Motion of Light and the Earth's annual Motion in its Orbit. For I perceived, that, if Light was propagated in Time, the apparent Place of a fixt Object would not be the ſame when the Eye is at Reſt, as when it is moving in any other Direction, than that of the Line paſſing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.

I conſidered this Matter in the following Manner. I imagined C A to be a Ray of Light, falling perpendicularly upon the Line B D; then if the Eye is at reſt at A, the Object muſt appear in the Direction A C, whether Light be propagated in Time or in an Inſtant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a Velocity that is to the Velocity of the Eye, as C A to B A; then Light moving from C to A, whilſt the Eye moves from B to A, that Particle of it,

it, by which the Object will be diſcerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I ſuppoſed the Line CB, to be a Tube (inclined to the Line BD in the Angle DBC) of ſuch a Diameter, as to admit of but one Particle of Light; then it was eaſy to conceive, that the Particle of Light at C (by which the Object muſt be ſeen when the Eye, as it moves along, arrives at A) would paſs through the Tube BC, if it is inclined to BD in the Angle DBC, and accompanies the Eye in its Motion from B to A; and that it could not come to the Eye, placed behind ſuch a Tube, if it had any other Inclination to the Line BD. If inſtead of ſuppoſing CB ſo ſmall a Tube, we imagine it to be the Axis of a larger; then for the ſame Reaſon, the Particle of Light at C, could not paſs through that Axis unleſs it is inclined to BD, in the Angle CBD. In like manner, if the Eye moved the contrary way, from D towards A, with the ſame Velocity; then the Tube muſt be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the viſible Place will not be ſo, ſince that, no doubt, muſt be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or leſs, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could ſuppoſe that Light was propagated in an Inſtant, then there would be no Difference between the real and viſible Place of an Object, altho' the Eye were in Motion, for in that caſe, AC being infinite with Reſpect to AB, the Angle ACB (the

Dif-

Difference between the true and viſible Place) vaniſhes. But if Light be propagated in Time (which I preſume will readily be allowed by moſt of the Philoſophers of this Age) then it is evident from the foregoing Conſiderations, that there will be always a Difference between the real and viſible Place of an Object, unleſs the Eye is moving either directly towards or from the Object. And in all Caſes, the Sine of the Difference between the real and viſible Place of the Object, will be to the Sine of the viſible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faſter than the Eye, and an Object (ſuppoſed to be at an infinite Diſtance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already ſaid, that the apparent Place of ſuch an Object will be always inclined to that Plain, in an Angle of 89° 56′½; ſo that it will conſtantly appear 3′½ from its true Place, and ſeem ſo much leſs inclined to the Plain, that way towards which the Eye tends. That is, if A C is to A B (or A D) as 1000 to one, the Angle A B C will be 89° 56′½, and A C B = 3′½, and B C D = 2 A C B = 7′. So that according to this Supposition, the viſible or apparent Place of the Object will be altered 7′, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at preſent ſuppoſe to be a Circle) as 1000 to one; then tis eaſy

to

to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, ſeem to change its Place continually, and (neglecting the ſmall Difference on the Account of the Earth's diurnal Revolution on its Axis) would ſeem to deſcribe a Circle round that Pole, every Way diſtant therefrom 3′ ½. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the ſame. Its right Aſcenſion would alſo change, and its Declination, according to the different Situation of the Sun in reſpect to the equinoctial Points; and its apparent Diſtance from the North Pole of the Equator would be 7′ leſs at the Autumnal, than at the vernal Equinox.

The greateſt Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the ſame, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given; the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only obſerved the apparent Difference of Declination of the Stars, I ſhall not now take any farther Notice in what manner ſuch a Cauſe as I have here ſuppoſed would occaſion an Alteration in their apparent Places in other Reſpects; but, ſuppoſing the Earth to move equally in a Circle, it may be gathered from what hath been already ſaid, that a Star which

is neither in the Pole nor Plain of the Ecliptick, will ſeem to deſcribe about its true Place a Figure, inſenſibly different from an Ellipſe, whoſe Tranſverſe Axis is at Right-angle to the Circle of Longitude paſſing through the Stars true Place, and equal to the Diameter of the little Circle deſcribed by a Star (as was before ſuppoſed) in the Pole of the Ecliptick; and whoſe Conjugate Axis is to its Tranſverſe Axis, as the *Sine* of the Stars Latitude to the *Radius*. And allowing that a Star by its apparent Motion does exactly deſcribe ſuch an Ellipſe, it will be found, that if A be the Angle of Poſition (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whoſe Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the ſame. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on Account of the Cauſe I am now conſidering) will be always as the Sine of the Sun's Longitude from thence. It will likewiſe be found, that the greateſt Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Tranſverſe Axis of the Ellipſe (or to the Semi-diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the ſame, is before

fore the Sun comes in Conjunction with or Oppoſition to it, if its Longitude be in the firſt or laſt Quadrant (*viz.* in the aſcending Semi-circle) of the Ecliptick; and after them, if in the deſcending Semi-circle; and it will appear neareſt to the North Pole of the Equator, at the Time of that *Maximum* (or when the greateſt Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

Theſe Particulars being ſufficient for my preſent Purpoſe, I ſhall not detain you with the Recital of any more, or with any farther Explication of theſe. It may be time enough to enlarge more upon this Head, when I give a Deſcription of the Inſtruments *&c.* if that be judged neceſſary to be done; and when I ſhall find, what I now advance, to be allowed of (as I flatter my ſelf it will) as ſomething more than a bare Hypotheſis. I have purpoſely omitted ſome matters of no great Moment, and conſidered the Earth as moving in a Circle, and not an Ellipſe, to avoid too perplexed a *Calculus*, which after all the Trouble of it would not ſenſibly differ from that which I make uſe of, eſpecially in thoſe Conſequences which I ſhall at preſent draw from the foregoing Hypotheſis.

This being premiſed, I ſhall now proceed to determine from the Obſervations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Suppoſition that the *Phænomena* before mentioned do depend upon the Cauſes I have here aſſigned. But I muſt firſt let you know, that in all the Obſervations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Preceſſion of

the Equinox, upon Suppoſition that the Alteration from this Cauſe is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Declination of each Star from the Obſervations themſelves; and I the rather chooſe to depend upon them in this Article, becauſe all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time 1″ ; or 2″ in a Year more than they would do if the Preceſſion was only 50″, as is now generally ſuppoſed. I have likewiſe met with ſome ſmall Varieties in the Declination of other Stars in different Years, which do not ſeem to proceed from the ſame Cauſe, particularly in thoſe that are near the ſolſtitial Colure, which on the contrary have altered their Declination leſs than they ought, if the Preceſſion was 50″. But whether theſe ſmall Alterations proceed from a regular Cauſe, or are occaſioned by any Change in the Materials *&c.* of my Inſtrument, I am not yet able fully to determine. However, I thought it might not be amiſs juſt to mention to you how I have endeavoured to allow for them, though the Reſult would have been nearly the ſame, if I had not conſidered them at all. What that is, I will ſhew, firſt from the Obſervations of γ *Draconis*, which was found to be 39″ more Southerly in the Beginning of *March*, than in *September*.

From what hath been premiſed, it will appear that the greateſt Alteration of the apparent Declination of γ *Draconis*, on Account of the ſucceſſive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would ſeem to deſcribe about the Pole of the Ecliptick, as 39″ to 40″,4. The half of this is the Angle A C B (as repreſented

ſented in the *Fig.*) This therefore being 20″, 2, A C will be to A B, that is, the Velocity of Light to the Velocity of the Eye (which in this Caſe may be ſuppoſed the ſame as the Velocity of the Earth's annual Motion in its Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ 12″.

It is well known, that Mr. *Romer*, who firſt attempted to account for an apparent Inequality in the Times of the Eclipſes of *Jupiter*'s Satellites, by the Hypotheſis of the progreſſive Motion of Light, ſuppoſed that it ſpent about 11′ Minutes of Time in its Paſſage from the Sun to us: but it hath ſince been concluded by others from the like Eclipſes, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypotheſis, is as it were a *Mean* betwixt what had at different times been determined from the Eclipſes of *Jupiter*'s Satellites.

Theſe different Methods of finding the Velocity of Light thus agreeing in the Reſult, we may reaſonably conclude, not only that theſe *Phænomena* are owing to the Cauſes to which they have been aſcribed; but alſo, that Light is propagated (in the ſame *Medium*) with the ſame Velocity after it hath been reflected as before: for this will be the Conſequence, if we allow that the Light of the Sun is propagated with the ſame Velocity, before it is reflected, as the Light of the *fixt Stars*. And I imagine this will ſcarce be queſtioned, if it can be made appear that the Velocity of the Light of all the *fixt Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Diſtances from them: both which points (as I apprehend) are ſufficiently proved from the apparent Alte-

ration

ration of the Declination of Stars of different Luſtre; for that is not ſenſibly different in ſuch Stars as ſeem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypotheſis) I find the ſame Velocity of Light from my Obſervations of ſmall Stars of the fifth or ſixth, as from thoſe of the ſecond and third Magnitude, which in all Probability are placed at very different Diſtances from us. The ſmall Star, for Example, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſteed*'s Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypotheſis, that the Diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick would be 40″, 2.

The laſt Star of the great Bear's-tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greateſt Alteration of Declination of a Star in the Pole of the Ecliptick would be 40″, 4, exactly the ſame as was before found from the Obſervations of γ *Draconis*.

The Star of the 5th magnitude in the Head of *Perſeus* marked τ by *Bayer*, was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be ſeen as it paſſes over my Zenith about the End of *June*, when it ſhould be according to the Hypotheſis fartheſt South. But becauſe I can more certainly depend upon the greateſt Alteration of Declination of thoſe Stars, which I have frequently obſerved about the Times when they become ſtationary,

nary, with reſpect to the Motion I am now conſidering; I will ſet down a few more Inſtances of ſuch, from which you may be able to judge how near it may be poſſible from theſe Obſervations, to determine with what Velocity Light is propagated.

α *Perſei Bayero* was 23″ more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be 40″, 2. α *Caſſiopeæ* was 34″ more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be 40″, 8. β *Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2. *Capella* was about 16″ more Southerly in *Auguſt* than in *February*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than thoſe I have before made uſe of, I cannot ſo well depend upon my Obſervations of it, as of the others; becauſe I meet with ſome ſmall Alterations of its Declination that do not ſeem to proceed from the Cauſe I am now conſidering.

I have compared the Obſervations of ſeveral other Stars, and they all conſpire to prove that the *Maximum* is about 40″ or 41″. I will therefore ſuppoſe that it is 40″$\frac{1}{2}$ or (which amounts to the ſame) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Obſervations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ ſo much as a Second from the Truth, and therefore it is probable that the Time which Light ſpends in paſſing from the Sun to us, may be determined by theſe Obſervations within 5″ or 10″; which is ſuch a degree of exactneſs as we can never hope to attain from the Eclipſes of *Jupiter*'s Satellites.

Having

Having thus found the *Maximum*, or what the greateſt Alteration of Declination would be in a Star placed in the Pole of the Ecliptick, I will now deduce from it (according to the foregoing Hypotheſis) the Alteration of Declination in one or two Stars, at ſuch times as they were actually obſerved, in order to ſee how the Hypotheſis will correſpond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to ſet down the whole Series of my Obſervations; I will therefore make Choice only of ſuch as are moſt proper for my preſent Purpoſe, and will begin with thoſe of *γ Draconis*.

This Star appeared fartheſt North about *September* 7th, 1727, as it ought to have done according to my Hypotheſis. The following Table ſhews how much more Southerly the Star was found to be by Obſervation in ſeveral Parts of the Year, and likewiſe how much more Southerly it ought to be according to the Hypotheſis.

1727. D	The Difference of Declination by Obſervation. "	The Difference of Declination by the Hypotheſis. "	1728. D.	The Difference of Declination by Obſervation. "	The Difference of Declination by the Hypotheſis. "
October 20th --	4 1/2	4 1/2	*March* - 24	37	38
November - 17	11 1/2	12	*April* - - 6	36	36 1/2
December - 6	17 1/2	18 1/2	*May* - - 6	28 1/2	29 1/2
- - - 28	25	26	*June* - - 5	18 1/2	20
1728			- - - 15	17 1/2	17
January - 24	34	34	*July* - - 3	11 1/2	11 1/2
February - 10	38	37	*Auguſt* - 2	4	4
March - - 7	39	39	*September* - 6	0	0

Hence

Hence it appears, that the Hypotheſis correſponds with the Obſervations of this Star through all Parts of the Year; for the ſmall Differences between them ſeem to ariſe from the Uncertainty of the Obſervations, which is occaſioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which cauſes the Stars ſometimes to dance to and fro, ſo much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glaſſes of the Teleſcope.

I muſt confeſs to you, that the Agreement of the Obſervations with each other, as well as with the Hypotheſis, is much greater than I expected to find, before I had compared them; and it may poſſibly be thought to be too great, by thoſe who have been uſed to Aſtronomical Obſervations, and know how difficult it is to make ſuch as are in all reſpects exact. But if it would be any Satisfaction to ſuch Perſons (till I have an Opportunity of deſcribing my Inſtrument and the manner of uſing it) I could aſſure them, that in above 70 Obſervations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypotheſis more than 2″, and this does not differ 3″.

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cauſe I have aſſigned, ſince the foregoing Obſervations make it ſufficiently evident, that the Effect of the real Cauſe, whatever it is, varies in this Star, in the ſame Proportion that it ought according to the Hypotheſis.

But leaſt γ *Draconis* may be thought not ſo proper to ſhew the Proportion, in which the apparent Altera-

tion of Declination is increaſed or diminiſhed, as thoſe Stars which lie near the Equinoctial Colure: I will give you alſo the Compariſon between the Hypotheſis and the Obſervations of *η Urſæ Majoris*, that which was fartheſt South about the 17th Day of *January* 1728, agreeable to the Hypotheſis. The following Table ſhews how much more Northerly it was found by Obſervation in ſeveral Parts of the Year, and alſo what the Difference ſhould have been according to the Hypotheſis.

1727.	d.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.	1728.	d.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.
		"	"			"	"
September	14	29 1/2	28 1/2	*April*	16	18 1/2	18
	24	24 1/2	25 1/2	*May*	5	24 1/2	23 1/2
October	16	19 1/2	19 1/2	*June*	5	32	31 1/2
November	11	11 1/2	10 1/2		25	35	34 1/2
December	14	4	3	*July*	17	36	36
1728							
February	17	2	3	*Auguſt*	2	35	35 1/2
March	21	11 1/2	10 1/2	*September*	20	26 1/2	26 1/2

I find upon Examination, that the Hypotheſis agrees altogether as exactly with the Obſervations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of ſo much as 2″, except in one, which is mark'd

mark'd as doubtful on Account of the Undulation of the Air, *&c.* And this does not differ 3″ from the Hypotheſis.

The Agreement between the Hypotheſis and the Obſervations of this Star is the more to be reguarded, ſince it proves that the Alteration of Declination, on account of the Proceſſion of the Equinox, is (as I before ſuppoſed) regular thro' all Parts of the Year; ſo far at leaſt, as not to occaſion a Difference great enough to be diſcovered with this Inſtrument. It likewiſe proves the other part of my former Suppoſition, *viz.* that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Preceſſion of 50″ would occaſion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Preceſſion was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Obſervations of thoſe Stars that lie near the Equinoctial Colure, at about the ſame Diſtance from the North Pole of the Equator, and nearly oppoſite in right Aſcenſion.

I think it needleſs to give you the Compariſon between the Hypotheſis and the Obſervations of any more Stars; ſince the Agreement in the foregoing is a kind of Demonſtration (whether it be allowed that I have diſcovered the real Cauſe of the *Phænomena* or not;) that the Hypotheſis gives at leaſt the true Law of the Variation of Declination in different Stars, with Reſpect to their different Situations and Aſpects with the Sun. And if this is the Caſe, it muſt be granted, that the Parallax of the fixt Stars is much ſmaller, than hath been hitherto ſuppoſed by thoſe,

who have pretended to deduce it from their Obſervations. I believe, that I may venture to ſay, that in either of the two Stars laſt mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I ſhould have perceived it, in the great number of Obſervations that I made eſpecially of γ *Draconis*; which agreeing with the Hypotheſis (without allowing any thing for Parallax) nearly as well when the Sun was in Conjunction with, as in Oppoſition to, this Star, it ſeems very probable that the Parallax of it is not ſo great as one ſingle Second; and conſequently that it is above 400000 times farther from us than the Sun.

There appearing therefore after all, no ſenſible Parallax in the fixt Stars, the *Anti-Copernicans* have ſtill room on that Account, to object againſt the Motion of the Earth; and they may have (if they pleaſe) a much greater Objection againſt the Hypotheſis, by which I have endeavoured to ſolve the fore-mentioned *Phænomena*; by denying the progreſſive Motion of Light, as well as that of the Earth.

But as I do not apprehend, that either of theſe Poſtulates will be denied me by the Generality of the Aſtronomers and Philoſophers of the preſent Age; ſo I ſhall not doubt of obtaining their Aſſent to the Conſequences, which I have deduced from them; if they are ſuch as have the Approbation of ſo great a Judge of them as yourſelf. I am,

Sir, Your moſt Obedient

Humble Servant

J. BRADLEY.

POSTSCRIPT.

AS to the Obſervations of Dr. *Hook*, I muſt own to you, that before Mr. *Molyneux*'s Inſtrument was erected, I had no ſmall Opinion of their Correctneſs; the Length of his Teleſcope and the Care he pretends to have taken in making them exact, having been ſtrong Inducements with me to think them ſo. And ſince I have been convinced both from Mr. *Molyneux*'s Obſervations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a Loſs how to account for it. I cannot well conceive that an Inſtrument of the Length of 36 Feet, conſtructed in the Manner he deſcribes his, could have been liable to an Error of near 30″ (which was doubtleſs the Caſe) if rectified with ſo much Care as he repreſents.

The Obſervations of Mr. *Flamſteed* of the different Diſtances of the Pole Star from the Pole at different Times of the Year, which were through Miſtake looked upon by ſome as a Proof of the annual *Parallax* of it, ſeem to have been made with much greater Care than thoſe of Dr. *Hook*. For though they do not all exactly correſpond with each other, yet from the whole Mr. *Flamſteed* concluded that the Star was 35″ 42″ or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypotheſis it ought to appear 40″ nearer in *December* than in *June*. The Agreement therefore of the Obſervations with the Hypotheſis is greater than could reaſonably be expected, conſidering the *Radius* of the Inſtrument, and the Manner in which it was conſtructed.

Jefferson's Efforts towards the Decimalization of United States Weights and Measures

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II. Early French Efforts to Establish a Decimal System of Weights and Measures.
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I. Manuscripts in the Library of the Department of State.
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I. DECIMALIZATION OF THE UNITED STATES COINAGE

1. The establishment and maintenance of some sort of currency to facilitate the exchange of commodities is one of the primary cares of an organized community. In the early days of independence in America, the necessity for systematization attracted the attention of great men, whose efforts resulted in the standardized decimal system of coinage of the United States. As early as 1777, by the Articles of Confederation (article IX), the Continental Congress was given « the sole and exclusive right and power of regulating the alloy and value of coin struck by their own authority, or by that of the respective states » (1).

Originally, the weights, measures and coins of the American colonies were based on those of the Mother Country. But during the Revolution, through clipping and sweating of coins, through

(1) *Articles of Confederation*, 3, (Williamsburg 1777).

REPORT

OF THE

SECRETARY OF STATE,

ON THE SUBJECT OF ESTABLISHING A UNIFORMITY

IN THE

WEIGHTS, MEASURES AND COINS

OF THE

UNITED STATES.

PUBLISHED BY ORDER OF THE HOUSE OF REPRESENTATIVES.

NEW-YORK:

PRINTED BY F. CHILDS AND J. SWAINE.

M,DCC,XC.

FIG. 1. Title page of the original edition of JEFFERSON's report, 1790. In this edition the two double lines on the title page are 84.5 mm. long. There are 50 pages. (Harvard College Library.) *Isis*, XVI, pl. 3.

II.

BUT if it be thought that, either now, or at any future time, the citizens of the United States may be induced to undertake a thorough reformation of their whole ſyſtem of Meaſures, Weights and Coins, reducing every branch to the ſame decimal ratio already eſtabliſhed in their Coins, and thus bringing the calculation of the principal affairs of life within the arithmetic of every man who can multiply and divide plain numbers, greater changes will be neceſſary.

THE unit of Meaſure is ſtill that which muſt give law through the whole ſyſtem: And from whatever unit we ſet out, the coincidencies between the old and new ratios will be rare. All that can be done will be to chuſe ſuch an unit as will produce the moſt of theſe. In this reſpect the ſecond rod has been found, on trial, to be far preferable to the ſecond pendulum.

MEASURES OF LENGTH.

LET the ſecond rod, then, as before deſcribed, be the ſtandard of Meaſure; and let it be divided into

FIG. 2. Page 39 of JEFFERSON's report, where he states the advantages of the decimal division for measures, weights and coins.

C. D. HELLMAN.

THE

American Accomptant;

BEING

A PLAIN, PRACTICAL AND SYSTEMATIC

COMPENDIUM

OF

FEDERAL-ARITHMETIC;

IN THREE PARTS:

DESIGNED FOR THE USE OF SCHOOLS,

AND SPECIALLY CALCULATED FOR THE

COMMERCIAL MERIDIAN

OF

THE UNITED STATES OF AMERICA.

BY *CHAUNCEY LEE*, A. M.

LANSINGBURGH:

PRINTED BY WILLIAM W. WANDS.

M,DCC,XCVII.

[*Published according to Act of Congress.*]

FIG. 3. The title page of LEE's *American Accomptant*. Opposite the title page are pictures of coins of various nations with the American Eagle of 1795 at the head. The line under « Chauncey Lee » is 41.0 mm. long. The book has xlii plus 297 pages. (Harvard College Library). Lansing-

This decimal table bears the following proportions to the vulgar table in general use, by which the reduction might be easily performed, *viz.*

Decimal Table.	*Gr.*	*Vulgar Table.*
1 Grain	= 1 =	1 Grain,
1 Scruple	= 10 =	10 Grains,
1 Dram	= 100 =	1 Dram and 2 Scruples,
1 Ounce	= 1000 =	2 Ounces and 2 Scruples,
1 Pound	= 10000 =	1 pound, 8 ounces, 5 drams.

Note. This table, perhaps, might be found rather inconvenient to the Apothecarist, in compounding his simples, until the Dispensatory could be conformed to this standard.

FEDERAL BOARD MEASURE.

The common Foot unaltered.

10 Pepper-Corns make 1 Inch,
10 Inches - - - - 1 Foot,
10 Square Feet - - 1 Pannel,
10 Pannels - - 1 Gang,
10 Gangs - - - 1 Thousand feet.

FEDERAL TABLE of things accounted by the *Tale*.

10 Particulars make 1 *Desm*,
10 Desms - - 1 Gross,
10 Gross - - - 1 Great Gross.

It may yet be an object of further inquiry whether the foregoing proposed reform in the doctrine of weights, might not be carried to a still higher pitch of simplicity and improvement; whether it might not answer a more beneficial purpose, not only to conform

FIG. 4. Page xxvi from LEE's book, giving decimal tables of weights and measures.

the outflow of metal due to the uneven balance of trade, through mixing with coins of other nations, uniformity of coinage no longer existed in America. Variations of monetary values from state to state made trading between them very difficult. Foreign coins increased the number of different coins to such an extent that few individuals were versed in the values of all the coins which might come into their hands.

2. Congress, upon hearing the report of a committee to whom was referred a motion relative to the weight and value of foreign coins, on January 7, 1782 ordered ROBERT MORRIS, Superintendent of Finance, to submit a report on this deplorable situation (2).

On January 15, 1782 a letter was sent to the President of Congress, officially signed by ROBERT MORRIS, which, aside from the required report on the rates at which foreign coins most likely to circulate within the United States should be received at the treasury, contained a plan for the establishment of a standard of value and the adoption of a money unit. JEFFERSON ascribes this letter to the Assistant Financier, GOUVERNEUR MORRIS, rather than to the Financier, ROBERT MORRIS (3).

3. In the letter (4) GOUVERNEUR MORRIS gives reasons for his plan, writing, « As we are now shaking off the inconveniences of a depreciating medium, the present moment seems to be that, in which a general currency can best be established, so as that in a few months, the same names of money will mean the same things in the several parts of the United States. » This would offer protection to the lower order of citizen. Another justification was the want of a legal tender. Distrust of the foreign nations whose coins were in circulation in the United States, and who might lower the pure metal content of their coinage, was also an argument used in favor of an American national standard.

One of the fundamental principles upon which MORRIS laid

(2) *Journals of Congress*, VII, 262 (New York 1787).

(3) *The Writings of Jefferson*, Library Edition (Washington 1903) I, *Autobiography*, 78. This letter is discussed in SPARKS' *Life of Gouverneur Morris*, I, 273-275 (Boston 1832), and by JEFFERSON in his *Autobiography*.

(4) *The Diplomatic Correspondence of the American Revolution*, edited by J. SPARKS, XII, 81-94 (Boston 1830) : also, *The Revolutionary Diplomatic Correspondence of the United States*, edited by F. WHARTON, V, 103-110 (Washington 1889) : also, *American States Papers, Documents, legislative and executive, of the Congress of the United States*, Class III, I, 101-103 (Washington 1832).

page are pictures of coins of various nations with the American Eagle of 1795 at the head. The line under « Chauncey Lee » is 41.0 mm. long. The book has still plus 297 pages. (Harvard College Library). Lansing-

more beneficial purpose, not only to conform

FIG. 4. Page xxvi from LEE's book, giving decimal tables of weights and measures.

his plan is that in order that a coin be properly intelligible to the whole people it must bear a close affinity to the former currency. For purposes of commerce he considered it necessary that the lowest divisible point of money, or *money unit*, be very small, although not necessarily exactly represented in coin, provided its precise value be known. The plan is further characterized by the statement, « *although it is not absolutely necessary, yet it is very desirable, that money should be increased in decimal ratio, because by that means all calculations of interest, exchange, insurance, and the like, are rendered much more simple and accurate, and of course, more within the power of the great mass of the people. Wherever such things require much labor, time, and reflection, the greater number who do not know, are made the dupes of the smaller number who do.* » (The italics have been added.)

Of the various coins circulating in America at the time of this report, the Spanish dollar, already requisite in many transactions, came nearest to furnishing a general standard. So MORRIS took the dollar for his basis of value. What he aimed to do, was to find a unit by which the coins in circulation could be estimated merely by having them assayed at the mint. To that effect he wrote, « In the present moment, it is by no means of such consequence to establish the relative value of different coins, as to provide a standard of our own, by which in future to estimate them. » It was in this letter that he strongly recommended the establishment of a mint, for, as he suggested, should the Spanish government introduce a greater alloy into their dollars, the American interior regulations as to money would be overturned.

MORRIS found that the money unit of a new system, to agree without a fractional remainder with all the different values of a dollar, excepting that in South Carolina, would be 1/1440th of a dollar, and would be equal to 1/4 of a grain of pure silver in coined money. There would be two copper coins, one of five units and one of eight. The lowest silver coin would be a *cent* or a hundred money units. From there the coins would increase in a decimal ratio.

4. This report was referred to a committe of the states, upon whose report Congress on February 21, 1782 (5) approved the

(5) *Journals of Congress*, VII, 286 (New York 1787).

establishment of a mint and directed the superintendent of finance to prepare and report a plan for establishing and conducting it. It is difficult to be precise about the next steps of the financier, as his reports are not available. (See Appendix I.)

April 30, 1783 (6) he again wrote to Congress further explaining and urging the unit he had proposed, but nothing more was done until the following year when the matter was referred to a committee of which THOMAS JEFFERSON was a member. JEFFERSON found that

« The general views of the Financier were sound, and the principle was ingenious on which he proposed to found his Unit; but it was too minute for ordinary use, too laborious for computation, either by the head or in figures.... Such a system of money-arithmetic would be entirely unmanageable for the common purposes of society. » (7)

JEFFERSON therefore proposed the adoption of the dollar as our Unit of account, putting its divisions and subdivisions in the decimal ratio, and submitting his notes on the subject to the consideration of the Financier. The latter's answer upheld his own general system, conceding only in taking as his unit one hundred of those he first proposed. To this JEFFERSON replied, printing his notes and reply on a « flying sheet », which he put into the hands of the members of Congress for consideration, and later incorporated in his autobiography.

5. JEFFERSON's notes (8) are clear and masterly, pointing to his ability as a scientist as well as a statesman. He stated three requisites of a money unit, (1) a convenient size for daily transactions, (2) having its parts and multiples in an easy proportion to each other, and (3) being, with its parts, so nearly of the value of some known coins as to be easily adopted by the people. He chose the Spanish dollar as most nearly fulfilling these conditions. As to the first requisite, he writes :

« Taking into our view all money transactions, great and small, I question if a common measure of more *convenient size* than the Dollar could be proposed. The value of 100, 1000, 10,000 dollars is well estimated by the mind; so is that of the tenth or hundredth of a dollar. Few transactions are above or below these limits. The expediency of attending to the size of the money Unit will

(6) *The Writings of Jefferson*, I, *Autobiography*, 78.

(7) Ibid., 78-79.

(8) Ibid., *Notes on the Establishment of a Money Unit, and of a Coinage for the United States*, 239 ff. : also, *American State Papers*, Class III, I, 105-107.

be evident, to any one who will consider how inconvenient it would be to a manufacturer or merchant, if, instead of the yard for measuring cloth, either the inch or the mile had been the Unit of Measure. »

To fulfill the second requisite, JEFFERSON chose the decimal ratio, as the easiest ratio of multiplication and divison, as already proposed by MORRIS. He went so far as to say that, « The bulk of mankind are school-boys through life.... And even mathematical heads feel the relief of an easier, substituted for a more difficult process. » He suggested the striking of four coins, a gold piece of ten dollars, a silver dollar, a silver tenth of a dollar, and a copper hundredth of a dollar. To uphold his argument, JEFFERSON compares some simple arithmetical operation on £ 8,13 s. 11 d. 1/2 qrs. and on 38.65 dollars. He then takes up the third requisite, comparing the values of the coins he has proposed with the values of known coins, and concluding that the people would accustom themselves to the new system with little difficulty. The Unit or Dollar was already a known coin. JEFFERSON writes :

« It is difficult to familiarize a new coin to the people; it is more difficult to familiarize them to a new coin with an old name. Happily, the dollar is familiar to them all, and is already as much referred to for a measure of value, as their respective provincial pounds. »

He suggests 3 additional silver coins because of the usefulness of small change and because of its tendency to reduce the prices of small articles. They would be, one of the value of a half dollar, one of two-tenths, and one of five coppers.

6. JEFFERSON discusses MORRIS' very small unit, saying :

« He was led to adopt this by a mathematical attention to our old currencies, all of which this Unit will measure without leaving a fraction. But as our object is to get rid of those currencies, the advantage derived from this coincidence will soon be past, whereas the inconveniencies of this Unit will forever remain, if they do not altogether prevent its introduction. It is defective in two of the three requisites of a Money Unit. 1. It is inconvenient in its application to the ordinary money transactions.... 2. It is neither equal, nor near to any of the known coins in value. »

Having determined upon the Dollar as the unit, it would then be necessary to state with precision the amount of pure silver in each dollar as determined by experimental examination of those in circulation, and also to fix the quantum of alloy. JEFFERSON's discussion of the metal content of the coins, and of the relation of silver to gold, does not concern us here. He urges Congress to take action on the subjects of a standard, a coinage, and a mint.

The controversy between JEFFERSON and MORRIS is, in the last analysis, whether to choose as the unit 1000 of the units Morris first proposed or the dollar. « We both agree that *the ease of adoption with the people*, is the thing to be aimed at », writes JEFFERSON in his *Notes*. His conclusion is :

« My proposition then, is, that our notation of money shall be decimal, descending *ad libitum* of the person noting; that the Unit of this notation shall be a Dollar; that coins shall be accommodated to it from ten dollars to the hundredth of a dollar; and that, to set this on foot, the resolutions be adopted which were proposed in the notes, only substituting *an enquiry into the fineness of the coins* in lieu of *an assay of them.* » (9)

(See Appendix II. for the proposed resolutions.)

7. The disorders in coins, weights and measures were a topic of general solicitude. MADISON remarked on them in a letter to MONROE dated April 28, 1785, (10) saying :

« I hear frequent complaints of the disorders of our coin, and the want of uniformity in the denominations of the States. Do not Congress think of a remedy for these evils? The regulation of weights and measure seem also to call for their attention. Every day will add to the difficulty of executing these works. If a mint be not established and a recoinage effected while the federal debts carry the money through the hands of Congress, I question much whether their limited powers will ever be able to render this branch of their perogative effectual. With regard to the regulation of weights and measures, would it not be highly expedient, as well as honorable to the federal administration, to pursue the hint which has been suggested by ingenious and philosophical men, to wit : that the standard of measure should be first fixed by the length of a pendulum vibrating seconds at the Equator or any given latitude; and that the standard of weights should be a cubical piece of gold, or other homogeneous body, of dimensions fixed by the standard of measure? Such a scheme appears to be easily reducible to practice; and as it is founded on the division of time, which is the same at all times and in all places, and proceeds on other data which are equally so, it would not only secure a perpetual uniformity throughout the United States, but might lead to universal standards in these matters among nations. Next to the inconvenience of speaking different languages, is that of using different and arbitrary weights and measures. »

This question of weights and measures will be discussed later.

8. In May 1785, came the « Report of the Grand Committee » on the money unit considering the reports of JEFFERSON and

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(9) *The Writings of Jefferson*, I, *Autobiography, Notes on the Establishment of a Money Unit, and of a Coinage for the United States*, 256.

(10) *Letters and other Writings of James Madison*, I, 152-3 (Philadelphia, by order of Congress, 1865).

MORRIS. They favored, without dispute, the decimal system for their money arithmetic (11).

On Wednesday July 6th of the same year the assembled Congress took into consideration the report of the grand committee, on the subject of a mint. The adoption of the dollar as the unit was carried by a unanimous vote. The smallest coin was set as a copper piece of 1/200 th dollar value (12).

9. Writing from Paris to WILLIAM CARMICHAEL on November fourth of that year, JEFFERSON summarizes this part of the work of Congress, thus, « They had begun arrangements for the establishment of a mint. The Dollar was decided on as the money Unit of America. I believe, they proposed to have gold, silver and copper coins, descending and ascending decimally; viz., a gold coin of ten dollars, a silver coin of one tenth of a dollar (equal to a Spanish bit), and a copper coin of one-hundredth of a dollar. These parts of the plan, however, were not ultimately decided on » (13).

10. While the committees were still arguing on the subject, MORRIS tried to formulate a decimal system which would more easily express the values of the coins in circulation and do away with the inconvenience of too small a unit. He chose as his unit an amount equal to twelve shillings, six pence sterling. The mode of reckoning in his system would not have been very different from that in the system of dollars and cents now in use. His idea was to have a money of account and a money of coinage (14). However, Congress rejected MORRIS's plan and adopted JEFFERSON's unit.

« But there are evidently two sides to the question, » wrote SPARKS in 1832 (15), « and it may not be easy even now to decide, which system was the best to attain the end desired in the shortest

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(11) WATSON, D. K., *History of American Coinage*, 19, (1899), quoting Manuscript Reports of the committee on Finance, Continental Congress, XXVI, 537-560.

(12) *Journal of Congress*, X, 225-6 (1785).

(13) *The Writings of Jefferson*, V, 197.

(14) SPARKS, J., *The Life of Gouverneur Morris*, with Selections from his correspondence and Miscellaneous Papers; detailing events in the American Revolution, the French Revolution, and in the Political History of the United States, I, 276-279, (Boston 1832).

(15) Ibid., 279-280.

time. As to the ultimate convenience, they would both have come to nearly the same thing. The primary object was to establish a uniform mode of reckoning money in all parts of the union; the next, to introduce a coinage, which should drive the old coins out of the country, and supersede their use. According to Mr Morris' plan, this was to be done by an entire new coinage, and a system of reckoning, which should adopt itself with the greatest facility both to the old coins, and to the different systems already in vogue. Mr Jefferson believed, that it would be more readily accomplished, by taking a standard coin, whose value the people well understood, and making it the basis of the new divisions and nomenclature. Everybody knew the value of a dollar, and could easily comprehend the parts of a dollar, let them be named as they might. » According to SPARKS, the fractional remainders in converting the old currencies into the new presented the same obstacles as MORRIS dreaded.

11. The Board of Treasury, on which were SAMUEL OSGOOD and WALTER LIVINGSTON, made three reports on April 8, 1786, favoring, among other points, the decimal system (16).

Four months later, August 8, 1786, (17) Congress adopted a complete decimal system of coinage, fixing the values and names of the coins. The standard was set at eleven parts fine gold or silver and one part alloy, as JEFFERSON had suggested. The decimal system was to operate on the mills, cents, dimes, and dollars, and the resolution also fixed the pure metal content of a Half Dollar, a Double Dime, a Half Cent, an Eagle (equal in value to ten dollars), and a Half Eagle. The mint price of gold and silver was fixed in terms of the new system. Congress, at the same time, ordered the board of treasury to report the draft of an ordinance for the establishment of a mint. October 16th (18) of the same year marked the third reading and the passing of such an ordinance. The mint was authorized to coin gold, silver, and copper money, agreably to the resolve of August 8th. The offices created were that of *Assay Master*,

(16) WATSON, D. K., *op. cit.*, 21-22, giving as reference Manuscript vol. CXXXIX, Letters and Reports of the Superintendent of Finance, Department of Rolls, 131-137, 139-142, 142-147, 151-175, 177-187.

(17) *Journal of Congress*, XI, 179-180.

(18) Ibid., 254-257.

Master Coiner, and *Paymaster*. The duties, which the offices would entail, are outlined. The officers are each to be bonded, and they and all persons employed in the mint are to take the oath of office. After September 1, 1787 no foreign coin would be considered current within the United States of America.

12. This was October 16th, 1786. November 7th JEFFERSON, in Paris, vastly interested in the subject, but still ignorant of its latest developments, wrote to M. DU RIVAL :

« Sir,—I am honored with your letter of the 1st instant, enclosing inquiries on the subject of the coins of the United States. Some time during the last year, Congress decided that the Spanish milled dollar should be their money unit, and that their coins should be in a decimal progression above or below that. Some intermediate coins will also be doubtless made for convenience, and indeed they determined that their smallest copper coin should be the two-hundredth part of a dollar. They did not determine how much pure silver their money unit should contain, nor establish the proportion between their silver and gold coins... » (18a)

This was done in the ordinance of October 16th which we have just discussed. But that was all, and no coins were struck, excepting a few pennies which were struck by one, JARVIS, who had a contract for copper coin (19).

13. In the ensuing years various reports were made concerning actual coining, but nothing more touched upon the decimal system, which is the point in which we are most interested.

April 7, 1790 a member of the House of Representatives from South Carolina presented to the House a letter from JOHN H. MITCHEL of that state giving the proposals of MATTHEW BOULTON of Great Britain for supplying the United States with copper coinage. The letter and proposals were referred to the Secretary of State, JEFFERSON (20). A week later, April 15th, the Speaker laid before the House the requested report and the letter accompanying it (21). In this report JEFFERSON stressed the opinion, in his own words, « that a mint, whenever established, should be established at home » (22).

(18a) *The Writings of Jefferson*, V, 460.

(19) MCMASTER, J. B., *A History of the People of the United States*, I, 403-404 (New York 1886).

(20) *Journal of the House of Representatives of the United States* (hereafter referred to as *Journal of the House*), I, 190 (Washington 1826).

(21) Ibid., 194.

(22) *American State Papers*, Class III, I, 44-45.

14. That same day, April 15, 1790, the House ordered the Secretary of the Treasury to prepare and report a proper plan or plans for establishing a National Mint (23). This was done, and well done. For, January 28, 1791, ALEXANDER HAMILTON, Secretary of the Treasury, communicated to the House his report on the establishment of a mint. The Speaker laid before the House both the report and the letter accompanying the report, which were read and ordered to lie on the table (24). The report is an exhaustive one, going over the ground already discussed in the previous reports and resolutions. HAMILTON takes up six points which are (1) the nature of the money unit of the United States, (2) the proportion between gold and silver, provided both metals are to be established, (3) the alloy, its proportion and composition, (4) the expense of the coinage, whether it be defrayed by the government or out of the material itself, (5) the number, denominations, sizes, and devices of the coins, and (6) whether foreign coins should be current or not, and if so, at what rate and for what period. HAMILTON was in favor of bimetalism, and his discussion of it is interesting (25). February 5th the report was ordered sent to the Senate (26), and that body appointed a committee on February 7th to consider it. March first MORRIS reported from that committee, the report being ordered to lie for consideration (27).

March 3, 1791 the House was informed that the Senate had agreed on the resolution, « That a mint shall be established, under such regulations as shall be directed by law.

« That the President of the United States be, and he is hereby, authorized to cause to be engaged, such artists as shall be necessary to carry the preceding resolution into effect, and to stipulate the terms and conditions of their service; and also to cause to be procured such apparatus as shall be requisite for the same

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(23) *Journal of the House*, I, 194.

(24) Ibid., 366.

(25) *American State Papers*, Class III, I, 91-100 : also *Reports of the Secretary of the Treasury*, I, 133 ff. (Washington 1829), which dates the report May 5, 1791.

(26) *Journal of the House*, I, 370.

(27) *Journal of the Third Session of the Senate of the United States of America*, 143 (Philadelphia 1791).

purpose. » Having inserted the word « principal » after the word « such », the House agreed to the resolution (28).

15. The first of November of the same year the committee of the Senate, appointed the preceding day to consider the necessary business, considered it proper for the the Senate to proceed in the establishment of a mint (29). Accordingly a bill entitled « An act establishing a mint, and regulating the coin of the United States » was introduced, and January 13, 1792, the Senate having passed it, it was read in the House for the first and second times and ordered committed to a Committee of the Whole House (30). March 25th JAMES MADISON wrote to EDMUND PENDLETON that the bill would receive a decision on its third reading in the House on the following day (31). But the next step seems to have been taken on March 24th, when the bill was discussed and amended (32). March 26th the bill, with the amendment, was read for the third time and passed, and the Clerk of the House was ordered to acquaint the Senate therewith (33). However, the Senate disagreed with the amendment proposed by the House, so that body reconsidered it the same day, nevertheless resolving to adhere to it (34). The following day, March 27th, the Senate sent a message that they receded from their disagreement to the amendment (35).

March 28th the bill was reported truly enrolled, and the Speaker signed it, whereupon the Clerk of the House was ordered to acquaint the Senate therewith (36). That day MADISON, writing to General LEE, reported the bill as having been passed on the previous day, and belittled the amendment which had caused so much trouble, and which was merely concerned with the stamping of the coin (37).

(28) *Journal of the House*, I, 402.

(29) *Journal of the Senate of the United States of America, being the First Session of the Second Congress*, 22 (Philadelphia 1791).

(30) *Journal of the House*, I, 491. The act is given in full in *Public Statutes at Large of the United States of America*, I, 246-251 (Boston 1850).

(31) JAMES MADISON, *op. cit.*, I, 550.

(32) *Journal of the House*, I, 547-549.

(33) Ibid., 549.

(34) Ibid., 550-551.

(35) Ibid., 551.

(36) Ibid., 553.

(37) JAMES MADISON, *op. cit.*, I, 551-552.

March 30th the Joint Committee for Enrolled Bills waited upon the President, and presented the bill for his approbation (38). He signed it April 2nd (39). And so, the Mint Act of April 2, 1792 came into being.

II. Early French Efforts to Establish a Decimal System of Weights and Measures (39a)

16. The French were long interested in decimalizing their weights and measures, and their efforts to adopt a metric system considerably antedate the efforts of the Americans (40). In fact, as early as 1670, Gabriel Mouton, Vicar of the Church of Saint Paul at Lyon, proposed a decimal system of measures based on the size of the earth (41). After numerous examples from the past, of the difficulties of preserving the length of measures, he sets forth a system of linear measures which he calls « *géométriques* », and which he adjusts to a decimal division, giving to the parts the names « *milliare*, *centuria*, *decuria*, *virga*, *virgula*, *decima*, *centisima*, *millesima* ». The « *milliare* », or thousand, was to equal the length of an arc of one minute of the great circle of the earth, so that the « *virga* » and « *virgula* », or 1/1000 and

(38) *Journal of the House*, I, 555.

(39) Ibid., 556.

(39a) Since completing this paper I have read the recent publication of Professor Adrien Favre entitled *Les Origines du Système Métrique* (Paris, 1931), which appeared too late to receive mention in the body of my article. However, Professor Favre emphasizes the standardization rather than the decimalization in his account of early French measures. His information concerning the American efforts is more incomplete than my own, and I had already seen all the sources which he mentions for the subject. For a more complete discussion of his book see my review in *Isis*.

(40) In 1585 the Plantin press of Leyden printed, in both Flemish and French, the *Thiende* by the Belgian, Simon Stevin, the French being a translation by the author himself and appearing under the title, *La Disme*, as an appendix to his *Arithmetique*. Stevin set forth two discoveries in this book, 1st that decimal fractions can be used exclusively in all computations, and 2nd that coins, weights and measures ought to be decimalized. Unfortunately, the second idea was not immediately followed up, and Stevin is not always given credit for it. For reference, see *La « Thiende » de Simon Stevin Facsimilé de l'édition originale Plantinienne de 1585 avec une introduction par H. Bosmans, S. J.* (Société des Bibliophiles Anversois édition n° 38) Anvers 1924, and also the review in *Isis*, 7 (1925) 543 by G[eorge] S[arton].

(41) Bigourdan, G., *Le Système Métrique des Poids et Mesures*, 6 (Paris 1901)

1/10000, would answer to the « toise » and foot (42). For an easy method of verifying these measures he connected them with the length of the second pendulum.

17. PICARD, the astronomer, considered the length of an oscillating pendulum as the basis of a system of measures (43), and until after the Revolution no new ideas were introduced (44), although a memoir by M. PRIEUR (DU VERNOIS) contained a plan for a decimal system and rejected the size of the earth in favor of the second pendulum as a standard (45).

18. In 1789 the Academy of Science appointed a committee to prepare a plan for bringing unity into the system of weights and measures (46). By this time interest was fully awakened and action was taken.

On February 7, 1790 the Marquis of BONNAY presented to the National Assembly a report prepared by the Royal Society of Agriculture at the request of the committee of agriculture and commerce, on the subject of uniformity in weights and measures. It was formulated by MM. TILLET and ABEILLE as observations on a memoir by M. DE VILLENEUVE, which aimed at the establishment of unity and conformity of measures in the whole kingdom (47).

19. At the session of the National Assembly on the 9th of March 1790 M. DE TALLEYRAND-PERIGORD, Bishop of Autun, presented a proposition on weights and measures (48). Not thinking these reflections suitable of being read at the Assembly, he had them printed and distributed to all the members. After describing the difficulties and injustice arising from unequal measures, TALLEYRAND states that the easiest method of bringing

(42) Idem, giving the reference « *Observationes diametrorum Solis et Lunae ... Huic adjecta est brevis dissertatio de ... nova mensurarum geometricarum idea.* Lugduni, in-4°, 1670. p. 427. »

(43) Ibid., 7.

(44) Ibid., 9.

(45) Idem, giving the reference « *Mémoire sur la nécessité et les moyens de rendre uniformes dans le royaume toutes les mesures d'étendue et de pesanteur; de les établir sur des bases fixes et invariables; d'en régler tous les multiples et les subdivisions suivant l'ordre décuple,* ... par M. Prieur (ci-devant Du Vernois), officier au corps royal du génie; Paris, 1790. »

(46) Ibid., 425.

(47) *Archives Parlementaires*, 1e Série, XI, 456, 466 ff., 808 (Paris 1880).

(48) Ibid., XII, 104-108 (Paris 1881).

uniformity would be to determine all the weights and measures of the kingdom on the basis of the double standard of the « livre » and the « toise » which were in Paris. But this is not what he recommends, saying :

« Lorsqu'une nation se détermine à opérer une grande réforme, il faut qu'elle évite, qu'elle redoute même de l'opérer à demi, pour n'être plus obligée d'y revenir ; et s'il s'agit d'une réforme dans les poids et mesures, il ne suffit pas de les réduire à un seul poids, à une seule mesure, comme on pourrait aisément le faire par le moyen indiqué ; il faut, pour que la solution du problème soit parfaite, que cette réduction se rapporte à un modèle invariable pris dans la nature afin que toutes les nations puissent y recourir dans le cas où les étalons qu'elles auraient adoptés viendraient à se perdre ou à s'altérer :.... »

He then presents two methods of arriving at a measure taken from nature. « La première », he writes, « consisterait à adopter pour élément de nos mesures linéaires, la soixante millième partie de la longueur du degré du méridien coupé en deux parties égales par le quarante-cinquième parallèle, et dont la longueur a été déterminée à 57,030 toises par M. de la Caille. Cette mesure élémentaire s'est trouvée avoir cinq pieds huit pouces cinq lignes un quart ; elle s'appellerait un ***milliaire***. Mille ***milliaires*** feraient un ***mille***, trois mille feraient une lieue, et vingt lieues composeraient un degré. Le ***milliaire*** tiendrait lieu de la toise dont il ne différerait que de quarante-deux lignes trois quarts, et se diviserait comme elle ***en six parties***, dont chacune représenterait un ***pied*** ». TALLEYRAND rejects this scheme in favor of the second, of which he says « La seconde méthode offre plus de facilités dans l'exécution. Ses nombreux partisans ont conseillé de prendre pour mesure élémentaire la longueur du pendule simple à secondes par la latitude de 45 degrés. Ils ont préféré ce point comme étant terme moyen entre l'équateur et le pôle : on donnerait alors à l'aune la longueur exacte de ce pendule, à notre toise le double de cette longueur, et la toise se subdiviserait en pieds, pouces et lignes, suivant les rapports connus de ces subdivisions ». This proposition did not in any way include an application of the decimal system, although it did discuss the use of a standard taken from nature.

In the proposition TALLEYRAND made the important suggestion that France and England should work together on the system, stating it thus, « Il m'est impossible de douter que l'Angleterre, qui dans ce moment paraît vouloir s'occuper de la réduction

de ses mesures, avertie par notre détermination et invitée par vous, ne se réunisse à la France pour l'exécution d'une entreprise que nos relations de commerce doivent rendre commune et dont le résultat doit appartenir un jour au monde entier ». And in the projected decree he added, « *Décrète* ensuite qu'il sera écrit par l'Assemblée nationale une lettre au Parlement d'Angleterre pour l'engager à concourir avec la France à la fixation de l'unité naturelle de mesures et de poids; qu'en conséquence, sous les auspices des deux nations, des commissaires de l'académie des sciences de Paris se réuniront en nombre égal avec les membres choisis de la société royale de Londres, dans le lieu qui sera jugé le plus convenable, pour déterminer à la latitude de 45 degrés la longueur du pendule, et en déduire un modèle invariable pour toutes les mesures et pour les poids; qu'après cette opération faite avec toute la solennité nécessaire, Sa Majesté sera suppliée de charger l'académie des sciences de fixer avec précision, pour chaque municipalité du royaume, les rapports de leurs anciens poids et mesures avec le nouveau modèle, et de composer ensuite, pour l'usage de chacune de ses municipalités, des livres usuels et élémentaires, où seront indiquées avec clarté toutes ces proportions. »

20. Two months after TALLEYRAND's proposition, on the 8th of May, the marquis of BONNAY, in the name of the committee of agriculture and commerce, reported on the uniformity to be established in weights and measures, and the Assembly passed a decree similar to the one proposed by TALLEYRAND (49).

In October a committee of the Academy suggested the adjustment of all weights, measures, and moneys to a decimal scale (50). Another report, dated March 19, 1791, made by a committee of the Assembly, preferred the length of a terrestrial arc to that of a second pendulum in founding the unit of measure (51). They chose the part of the meridian between Dunkirk and Barcelona (52).

21. Thus by the spring of 1791 the main points of the French

(49) Ibid., XV, 438 ff., 443, 792-793 (Paris 1883). BIGOURDAN in his chronological table gives May 6th for the date of DE BONNAY's report.

(50) BIGOURDAN, G., *op. cit.*, 16-17.

(51) Ibid., 17-18 and footnotes.

(52) Ibid., 19.

metric system had been discussed. The cooperation of England with France in this work was not the success that TALLEYRAND had anticipated, not because the subject of weights and measures did not affect the English, but because they worked alone and the adoption of a decimal system does not seem to have been considered by their parliament. However, they had already taken measures to rectify the existing abuses in the middle of the 18th century, and in 1758 a report was made from the committee appointed to inquire into the original standards of weights and measures in the kingdom and to consider the laws relating thereto (53).

22. In this report the committee discussed and compared in great detail the different weights and measures in the kingdom and the laws relating thereto, and presented resolutions, the adoption of which would render uniform and fixed *the weights then in use*. In the Appendix number I to their report is given an account of the acts in the reign of Queen ELIZABETH in connection with weights and measures, and in number II the acts in the reigns of the sovereigns of England from the time of HENRY the 3RD to the time of GEORGE 2ND.

In 1759 a report was presented by the committee appointed December 1, 1758 to inquire into the original standards of weights and measures in the kingdom, and to consider the laws relating thereto (54). This report added nothing new to the theory of weights and measures and did not mention the two points which were so important in the American and French discussions of thirty years later, namely, the standard taken from nature and the subdivisions and multiples in the decimal ratio. However, these two reports formed the basis for further discussion in Parliament when the subject of weights and measures was brought up in 1789 and 1790.

24. July 24, 1789 Sir JOHN RIGGS MILLER stated in a parliamentary speech on weights and measures that he had addressed a circular letter to the magistrates of the cities, corporations,

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(53) *Reports from Committees of the House of Commons, which have been printed by Order of the House*, and are not inserted in the Journals. Reprinted by Order of the House, II, 411-451 *Report relating to Weights and Measures*, June 2, 1758.

(54) Ibid., 453-463 *Another Report relating to Weights and Measures*, April 12, 1759.

and borough towns asking for information on the subject. Sir JOHN stressed the necessity of equalizing weights and measures (55).

February 5, 1790 Sir JOHN again addressed the House on the same subject. He took up the following five points, (1) the uncertainty and perplexity in the weights and measures of the country, (2) the causes, which in the circumstance and conditions of the legal standards, make the uncertainty and perplexity permanent and inevitable, (3) the mischievous influence of the inequality of the weights and measures on commerce and on the comforts of individuals as well as on the community, which topic was to be discussed on a future day, (4) the offering of some immediate corrections of these prevailing abuses, and (5) the suggestion of some general standard to which all weights and measures might be referred, itself derived from something invariable and immutable in nature at all times and in all places equal and the same. Sir JOHN hoped that the « vibrations of a pendulum » would prove such a standard. He ended the speech by moving two resolutions which were unanimously agreed to, namely (1) « That the clerks of the market of the different cities and market towns throughout England and Wales, and the town of Berwick-upon-Tweed, and the clerks of the different counties in the same, do forthwith make out and transmit to the sheriffs of the respective counties in which the said towns are situated, returns of the different weights and measures now in use in their respective cities and market towns, as well as specifications and descriptions of any particular commodities that are bought and sold by any customary denominations or proportions of weight and measure, as far as any such have come under their observations » and (2) « That the said order be sent to the sheriffs of the several counties in England and Wales, and be by them transmitted to the clerks of the markets in their respective counties; and that the said sheriffs do return to the clerk of this House, to be by him laid before the House, the returns they shall receive from the clerks of the markets » (56). On April first the Speaker

(55) *The Parliamentary History of England*, from the earliest period to the year 1803. from which last-mentioned epoch it is continued downwards in the work entitled, « The Parliamentary Debates ». XXVIII, 297 ff. (London, Printed for T. C. HANSARD, 1816).

(56) Ibid., XXVIII, 315 ff. The two orders are also given in the *Journals of the House of Commons*, XLV, 61 for February 5, 1790.

acquainted the House that several returns to the above orders had been received (57), and a committee, of which Sir JOHN MILLER was a member, was appointed to consider the returns and to report their observations and opinion thereupon (58). April 13th the reports of 1758 and 1759 were referred to the same committee (59), upon the motion made by Sir JOHN MILLER at the close of his speech that day (60). In this speech he acknowledged the receipt of a private letter accompanied by a printed proposition addressed to the national assembly from TALLEYRAND, Bishop of Autun. This is no doubt the proposition previously discussed in this section. However, it brought no new results, Sir JOHN stressing that « The bishop himself, indeed, had very liberally acknowledged in his private letter, that he took the hint of making his proposition to the national assembly of France from what had been lately submitted to the British parliament upon the same subject », and saying that « he should not avail himself of any new lights afforded by the Bishop of Autun's proposition ». It might be interesting to imagine what would have been the result if the French and English nations had united in an effort to bring uniformity into their systems of weights and measures. Sir JOHN elaborates on the five points of his last speech on weights and measures, particularly the third, and describes the evil results of the existing inequalities. He then says that there are two kinds of qualities of a standard, the « essential » and the « eligible ». The essential was that the standard be taken from something permanent and uniform in nature. Among eligible qualities were that it be of sufficiently large dimensions, « that », in Sir JOHN's words, « its denominations be in tens, to give it the advantage of whole numbers, or decimal fractions », that it be connected with two things in nature, that it correspond in round numbers with some of the weights and measures then in use, and that it correspond with weights and measures in neighboring nations. He suggested four standards taken from nature, the first taken from a drop of water, the second

(57) *Journals of the House of Commons*, XLV, 351-352 (Printed by Order of the House of Commons).

(58) Ibid., 359.

(59) Ibid., 388.

(60) HANSARD, T. C., *op. cit.*, XXVIII, 639 ff.

from the space traversed by falling bodies in a second of time, the third from a degree of a great circle of the earth, and the fourth from the length of a pendulum, saying « the pendulum which vibrates seconds at London, is the most proper standard for Great Britain, and a medium for all Europe » (61). Thus finally we have mention, if only in passing, of the introduction of a decimal system into English weights and measures, and also of the great circle of the earth and the length of a pendulum as furnishing the standard.

25. It is hard to say exactly how much JEFFERSON obtained from either the French or the English in formulating his plan for a decimal system of weights and measures in the United States. The possibility of his plan coming from England is negligible; and as we have seen above, the decimalization of weights and measures was not actively discussed in France during JEFFERSON's stay there, 1784 to September 1789, although it had been previously suggested. Would it not be just as logical to suppose that JEFFERSON was led to propose a decimal division in weights and measures through his interest in establishing a decimal coinage system? The English interest in weights and measures in 1789 and 1790 seems to have been entirely in standardization, decimalization being only casually mentioned. Nevertheless, the idea of a decimal division of weights and measures was current there, as we shall see from a book printed in London in 1790, but taken from an earlier manuscript (62). Thus we may suppose that the notion was a general one, and there seems to be no need to ascribe JEFFERSON's reception of it to a definite place. It cannot be denied that his interest in the coinage must have played an important part in developing the notion, no matter whence he had received it.

The book mentioned in the preceding paragraph, although having little effect at the time, is worthy of mention here. It is entitled « A Plan For Introducting an Uniformity of Weights and Measures », was written by Sir JAMES STEUART, Bart. and was published after his death by his son. His decimal plan is here quoted :

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(61) Idem.

(62) *A Plan for introducing an uniformity of weights and measures within the limits of the British Empire* by the late Sir JAMES STEUART (London 1790).

« Fractions, then, being unavoidable, those called decimals have appeared to many, and certainly are, the best, for expressing with distinctness, all different denominations; and were it possible to introduce them into the new measures, it would be a great additional improvement.

« For the easier accomplishment of this part of the plan, I should propose to reject entirely, as standards, any statute measures now in use, and to take a mass *ad libitum* for the standard of weight, and the London pendulum for the standard of lineal extension.

« These may be subdivided into decimal parts :... ». (63)

27. The main features of JEFFERSON's plan had been completed before he received either the proposition of TALLEYRAND or the speech of Sir JOHN RIGGS MILLER of the 13th of April 1790. This we know conclusively both from his letter to DAVID RITTENHOUSE on June 14th of that year (64) and from the letter to the Speaker of the House of Representatives accompanying the report (65). The changes which JEFFERSON made in his report because of TALLEYRAND's proposition affect only the details, not the theory. He speaks of them in both of the above mentioned letters, writing to RITTENHOUSE :

« I enclosed you the day before yesterday a rough draught of the report I had prepared on the subject of weights and measures. I have this morning received from Mr. Short a proposition made by the Bishop of Autun to the National Assembly of France, on the same subject, which I enclose you, and will beg the favor of you to return it by post after you shall have perused it. He mentions that the latitude of 45°, as being a middle term between the equator and pole, had been proposed as the general standard for measures, and he makes the proposition anew, and desires it may be made to England. As this degree of latitude is our northern boundary, as it may form a link between us and Europe, and as the degree which shall otherwise give the standard is not otherwise very material, I have thought of proposing it in my report instead of the 38th degree. I have in consequence gone over my calculations again upon the ground of a pendulum of 36. -8.428. (Sir Isaac Newton's calculation for 45°) 39.14912 inches giving a rod of 58.72368 inches, and reformed the tables (last page of the report), of which reformation I send you a copy. The alterations in the body of the work may be easily made from this. The Bishop says the pendulum has been calculated for 45° to be 36. -8.52. this 1/10 of a line less than Sir Isaac Newton's, and the Bishop accordingly adds, that there may be in this calculation an error of 1/10 of a line. »

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(63) Ibid., 19-20.

(64) *The Writings of Jefferson*, VIII, 37-38 Letter to DAVID RITTENHOUSE, New York, June 14, 1790.

(65) JEFFERSON, T., *Report of the Secretary of State, on the subject of establishing a uniformity in the Weights, Measures and Coins of the United States* iii-iv (New York, Published by order of the House of Representatives, 1790).

In the above mentioned letter to the Speaker of the House JEFFERSON writes :

« In the Report which I had prepared, and was then about to give in, I had proposed the latitude of 38°, as that which should fix our standard, because it was the medium latitude of the United States; but the proposition before the National Assembly of France, to take that of 45° as being a middle term between the Equator and both Poles, and a term which might consequently unite the nations of both hemispheres, appeared to me so well chosen, and so just, that I did not hesitate a moment to prefer it to that of 38°. »

28. On July 26, 1790 JEFFERSON wrote to WILLIAM SHORT from New York, acknowledging receipt of TALLEYRAND's proposition, stating what changes it had occasioned in his own plan, and enclosing copies of his report for TALLEYRAND and for CONDORCET, Secretary of the Academy of Sciences. He added, « By taking the second pendulum or rod of the same latitude, for the basis of our measures, it will at least furnish a common measure to which both our systems will refer, provided our experiments on the pendulum or rod of forty-five degrees should yield exactly the same result with theirs » (66).

29. JEFFERSON did not approve of the system the French finally adopted and put into effect, and he expressed himself on the subject in another letter to SHORT just a year later, dated from Philadelphia, July 28, 1791. In this letter he wrote :

« The element of measure adopted by the National Assembly excludes, *ipso facto*, every nation on earth from a communion of measure with them; for they acknowledge themselves, that a due portion for admeasurement of a meridian crossing the forty-fifth degree of latitude, and terminating at both ends in the same level, can be found in no other country on earth but theirs. It would follow then, that other nations must trust to their admeasurement, or send persons into their country to make it themselves, not only in the first instance, but whenever afterwards they may wish to verify their measures. Instead of concurring, then, in a measure which, like the pendulum, may be found in every point of the forty-fifth degree, and through both hemispheres, and consequently in all countries of the earth lying under that parallel, either northern or southern, they adopt one which can be found but in a single point of the northern parallel, and consequently only in one country, and that country is theirs. » (67)

What JEFFERSON admired most in the French plan was the courage with which the innovation was carried into effect, as he wrote to J. DORSEY in a letter dated Washington, January 21, 1808, when he said, « It is a great and difficult question whether

(66) *The Writings of Jefferson*, VIII, 69-70.
(67) Ibid., VIII, 220-221.

to venture only on a half reformation, which by presenting fewer innovations, may be more easily adopted, or, as the French have tried with success, make a radical reform » (68). In September 1811 JEFFERSON, writing to Dr. ROBERT PATTERSON, again criticized the French system, and expressed the hope « that the other nations of the world will still concur, some day, in making the pendulum the basis of a common system of measures, weights and coins, which applied to the present metrical systems of France and of other countries, will render them all intelligible to one another »(69). The following month, again writing on the subject to Dr. PATTERSON, JEFFERSON objected to the arc of the meridian between Barcelona and Dunkirk, which through friendly relations between France and Spain, the French had been able to measure (70).

30. When JOHN QUINCY ADAMS was preparing a *Report upon Weights and Measures*, in obedience to a resolution of the Senate of the third of March, 1817 (71) JEFFERSON wrote to him from Monticello on November 1, 1817 (72). He again voiced his objections to the French standard and spoke of the recent reform in England in the following terms,

« But the cogent reason which will decide the fate of whatever you report is, that England has lately adopted the reference of its measures to the pendulum. It is the mercantile part of our community which will have most to do in this innovation; it is that which having command of all the presses can make the loudest outcry, and you know their identification with English regulations, practices, and prejudices. It is from this identification alone you can hope to be permitted to adopt even the English reference to a pendulum. But the English proposition goes only to say what proportion their measures bear to the second pendulum of their own latitude, and not at all to change their unit, or to reduce into any simple order the chaos of their weights and measures ».

JEFFERSON continues his letter by urging ADAMS to propose

(68) Ibid., XI, 426-8. Bulletin 6 of the Bureau of Rolls dates this letter January 27, 1808.

(69) *The Writings of Jefferson*, XIII, 85-89.

(70) Ibid., XIII, 95-108.

(71) ADAMS, JOHN QUINCY, *Report upon Weights and Measures* (Philadelphia 1821).

(72) *The Writings of Jefferson*, XV, 145-149.

a unit with decimal divisions, the convenience of which he demonstrates by pointing to the moneyed system.

Thus JEFFERSON without losing his individuality was able to pick out the best from the work of the foreign nations in the field of weights and measures.

III. JEFFERSON AND EARLY AMERICAN EFFORTS TO ESTABLISH A DECIMAL SYSTEM OF WEIGHTS AND MEASURES

31. Although the Articles of Confederation of 1777 had granted to Congress the right of fixing the standard of weights and measures, (73) no immediate action took place. We have already mentioned MADISON's letter to Monroe (§ 7.) on April 28, 1785 in which he urged the fixing of a standard of weights and measures and expressed himself in favor of the second pendulum as furnishing the measure of length. On August 19th of the same year the Continental Congress ordered the board of treasury to report an ordinance fixing the standard of weights and measures throughout the United States (74). But that, too, seems to have had little effect.

In 1787 the Federal Constitution confirmed the right of Congress to fix the standard of weights and measures (75). Then in 1790 more decisive steps were taken. On January 8th in the first annual address of the President to Congress, GEORGE WASHINGTON spoke of the importance of uniformity in the currency, weights, and measures, and said he was persuaded it would be attended to (76). Accordingly January 15th the House of Representatives ordered the Secretary of State to prepare and report « a proper plan or plans for establishing uniformity in the currency, weights, and measures, of the United States » (77).

32. The Secretary of State was THOMAS JEFFERSON. He did not receive the order until April 15th and after that an illness caused a few weeks' delay. It was about the 20th of May that

(73) *Articles of Confederation*, 3.

(74) *Journal of Congress*, X, 271 (1785).

(75) *The Fedaral Constitution*, Clause 5 Section 8 Article I.

(76) WASHINGTON, GEORGE, in *Congressional Register*, III, 7 (New York 1790).

(77) *Journal of the House*, I, 141-142 : also JEFFERSON, *Report on Weights and Measures*, vii. The *Congressional Register* erroneously states that the subject was referred to the Secretary of the Treasury (III, 107).

he finished the report, and then he withheld it, as he said, to « lessen the number of its imperfections » (78).

June 12th JEFFERSON wrote to both DAVID RITTENHOUSE (79) and ALEXANDER HAMILTON (80) enclosing drafts of his report. From HAMILTON he received a letter expressing satisfaction with it (81). His letter to RITTENHOUSE started a long correspondence on the subject, and is exceedingly interesting. It is given in extenso in the Appendix (number III) to this paper. June 14th JEFFERSON received TALLEYRAND's proposition. We have already quoted (§ 27.) the letter relating to it which he wrote the same day to RITTENHOUSE. In this letter he speaks of taking « no notice of the precaution of making the experiment of the pendulum on the sea shore, because the highest mountain in the United States would not add 1-5000 part to the length of the earth's radius, nor 1-128 of an inch to the length of the pendulum; .... » June 17th he again wrote to RITTENHOUSE, this time enclosing alterations to his report (82). June 18th the speech of Sir JOHN RIGGS MILLER in Parliament reached JEFFERSON (83). June 20th he wrote another letter to RITTENHOUSE, discussing the details of the report. This letter is also included in the Appendix (number IV). RITTENHOUSE acknowledged the letters of the 12th, 14th and 17th on the 21st, and on the 25th again wrote to JEFFERSON on weights and measures (84). June 26th JEFFERSON acknowledged RITTENHOUSE's letter of the 21st and June 30th that of the 25th (85). July 2nd RITTENHOUSE wrote to JEFFERSON comparing the measure of the French and English foot, discussing the practical difficulty resulting from the effect of the machinery and moving power, giving a demonstration as applied to the pendulum, and speaking of the probable length of a degree of

(78) JEFFERSON, T., *Report on Weights and Measures*, iii Letter to the Speaker of the House.

(79) DAVID RITTENHOUSE (1732-1796), maker of clocks and astronomical instruments, President of the American Philosophical Society, Director of the Mint.

(80) *Bureau of Rolls and Library, Bulletin* 6, 197.

(81) Ibid., *Bulletin* 8, 285.

(82) Ibid., *Bulletin* 6, 392 : also *The Writings of Jefferson*, VIII, 38-41, letter to RITTENHOUSE, June 20, 1790.

(83) *The Writings of Jefferson*, VIII, 38-41, letter to RITTENHOUSE, June 20, 1790.

(84) *Bureau of Rolls and Library, Bulletin* 8, 472.

(85) Ibid., *Bulletin* 6, 392.

longitude (86). Then, August first JEFFERSON wrote enclosing the report (87). Thus it seems that RITTENHOUSE is responsible, if not for many of the details, at least for confirmation of them.

33. While engaged in writing this report JEFFERSON learned of the oscillating rod thought of by ROBERT LESLIE, the watchmaker, for a standard of measures. He procured a manuscript pamphlet of LESLIE's on this subject from MADISON. Then on June 27, 1790 he wrote to LESLIE asking for permission to propose the rod in preference to the pendulum in his report, where he would state his indebtedness to LESLIE for the idea. In case of the adoption of the plan, he wrote, there might be occasion to engage LESLIE's assistance in the proper adjustment of it (88).

34. JEFFERSON's letter to the Speaker of the House accompanying his report is dated July 4, 1790 (89). July 13th the Speaker laid JEFFERSON's letter covering his report before the House and it was read and ordered to lie on the table (90).

35. In his exhaustive report (91) JEFFERSON first states the necessity of finding some measure of invariable length to serve as a standard with which the measures, weights and coins could be compared. Finding no such standard in matter by its mere extension, he chooses its motion. Thus he discards the standard provided by an admeasurement of a great circle of the earth. For the motion of matter he uses the motion of the earth on its axis which, he says, « though not absolutely uniform and invariable, may be considered as such for every human purpose ». The measure of determinate length upon which he fixes is derived from this motion and is the pendulum oscillating seconds of mean time. He lists the following five sources of uncertainty in the pendulum : (1) The difficulties to be met include that of ascertaining its center of oscillation as depending on the form of the bob and its distance from the point of suspension; the displacement of this center due to the weight of the suspending

(86) Ibid., *Bulletin* 8, 472.
(87) Ibid., *Bulletin* 6, 392.
(88) *The Writings of Jefferson*, VIII, 48-49.
(89) JEFFERSON, T., *Report on Weights and Measures*, iii.
(90) *Journal of the House*, I, 268.
(91) JEFFERSON, T., *Report on Weights and Measures*. The report is also given in *American State Papers*, Class X, I, 13-20.

wires; and the fact that the center seated within the bob would be inaccessible to the measure. (2) In order to preserve its isochronism it must be shorter towards the equator and longer towards the poles. (3) The length of the pendulum is diminished by the height of the situation above the common level, which acts as an increment to the radius of the earth. (4) There are variations in a metal pendulum due to variations in temperature. And (5) in JEFFERSON's own words, « To continue small and equal vibrations, through a sufficient length of time, and to count these vibrations, machinery and a power are necessary, which may exert a small, but constant effort to renew the waste of motion : And the difficulty is, so to apply these, as that they shall neither retard nor accelerate the vibrations ». JEFFERSON then proceeds to answer these objections in order : (1) By substituting for the pendulum an uniform cylindrical rod without a bob, as proposed by Mr. LESLIE of Philadelphia, it is possible to avoid the uncertainties respecting the center of oscillation. Then, to all intents and purposes, the center of oscillation could be considered as residing 2/3 of the distance from the center of suspension. (2) The difference in length of the second rod based on the differences of latitude could be overcome by fixing on some one latitude as a standard. Considering the United States alone, 38° as the middle latitude would be the most convenient. Taking other nations into consideration, 45°, as the middle term between the equator and the poles, is more suitable. 45° is also distinguished in the United States by being the principal northern boundary. If observations are made in another latitude, it is possible by computation to bring them exactly to the standard of 45°. (3) The experiment can be made in the level of the ocean, to avoid the difference which a higher position might occasion. (4) Expansion and contraction of the rod with changes of temperature may be avoided by keeping the standard in a cellar or similar place of invariable temperature. Iron, as the least expansible of the metals, is suggested for this purpose. (5) The difficulties arising from the machinery to propel and count the strokes of the pendulum must be met by the artist.

36. According to the report an advantage of the rod over the pendulum is that in the event of fixing the foot at the nearest aliquot part of either, the difference between the common foot

and the deduced foot would be three times greater if the pendulum were adopted than if the rod were chosen. The conclusion of the report as to the standard is,

« Let the standard of measure then be an uniform cylindrical rod of iron, of such length, as, in latitude 45° in the level of the ocean, and in a cellar, or other place, the temperature of which does not vary through the year, shall perform its vibrations, in small and equal arcs, in one second of mean time. »

By that standard the measures, weights, and coins of the United States can be identified.

37. The next question concerns the extent of reformation contemplated by the House of Representatives, and the report continues :

« The experiment made by Congress in the year 1786, by declaring that there should be one money of account and payment through the United States, and that its parts and multiples should be in a decimal ratio, has obtained such general approbation, both at home and abroad, that nothing seems wanting, but the actual coinage, to banish the discordant pounds, shillings, pence, and farthings of the different states, and to establish in their stead, the new denominations. Is it in contemplation with the House of Representatives to extend a like improvement to our Measures and Weights, and to arrange them also in a decimal ratio? The facility which this would introduce into the vulgar arithmetic, would, unquestionably, be soon and sensibly felt by the whole mass of the people, who would thereby be enabled to compute for themselves whatever they should have occasion to buy, to sell, or to measure, which the present complicated and difficult ratios place beyond their computation, for the most part. Or, is it the opinion of the Representatives that the difficulty of changing the established habits of a whole nation opposes an insuperable bar to this improvement? Under this uncertainty the Secretary of State thinks it his duty to submit alternative plans, that the House may, at their will, adopt either the one or the other, exclusively, or the one for the present, and the other for a future time, when the public mind may be supposed to have become familiarized to it. »

The first part of the remainder of the report is based on the supposition that the weights and measures then in use were to be retained but made uniform and invariable by adjusting them to the same invariable standard.

The only weights and measures generally established in the United States were those brought over from England by the early settlers. To fix these it was necessary to turn to England for information, and the reports of the committees of the House of Commons in 1758 and 1759 were considered by JEFFERSON as the best written testimony of the standard weights and measures of that country. JEFFERSON continues by enumerating the measures of length in use in the United States, as follows :

« The League of 3 miles;
The Mile of 8 furlongs;
The Furlong of 40 poles or perches;
The Pole or Perch of 5 1/2 yards;
The Fathom of 2 yards;
The Ell of a yard and quarter;
The Yard of 3 feet;
The Foot of 12 inches;
And the Inch of 10 Lines ».

From the English reports JEFFERSON took the statements that there were in deposit at the Exchequer a yard supposedly of the time of HENRY VII and a yard and ell supposedly made about 1601, and that they were brass rods, coarsely made, bent, and with inexact divisions. There was also a brass rod agreeing as nearly as possible with the standards at the Exchequer, which had been made in 1742, pursuant to the experiments of some members of the Royal Society, who had taken great pains in measuring those standards with some curious instruments prepared by GEORGE GRAHAM. The means of knowing what this standard is are furnished to people at a distance by the evidence of the second pendulum. According to NEWTON the length of the second pendulum at London is 39.1682 English inches, and consequently that of the second rod would there be 58.7523 of the same inches. JEFFERSON continues by saying :

« When we shall have found, then, by actual trial, the second rod for 45° by adding the difference of their computed length [this JEFFERSON had given earlier in the report as equal to 58.72368 English inches], to wit 287/10,000 of an inch, or rather 3/10 of a line (which in practice will endanger less error, than an attempt at so minute a fraction as the ten thousandth parts of an inch) we shall have the second rod of London, or a true measure of 58 3/4 English inches. »

To shorten the operation he suggests dividing the standard rod of 45° into 587 1/5 equal parts, each of which would be declared a line, and finding the other measures from the line. This would define the inch in terms of the standard rod he had proposed.

JEFFERSON suggests that the surface measures, established as the acre of 4 roods and the rood of 40 square poles, should remain the same.

His next step is to list the measures of capacity as follows :

« The gill, 4 of which make a pint;
2 Pints make a quart;
2 Quarts a pottle;

2 Pottles a gallon;
2 Gallons a peck, dry measure;
8 Gallons make a measure called a firkin, in liquid substances, and a bushel, dry;
2 Firkins or bushels, make a measure called a rundlet, or kilderkin, liquid, and a strike, dry;
2 Kilderkins, or strikes make a measure called a barrel, liquid, and a coomb, dry; this last term being ancient and little used;
2 Barrels, or coombs make a measure called a hogshead, liquid, or a quarter, dry; each being the quarter of a ton;
A Hogshead and a third make a tierce, or third of a ton;
2 Hogsheads make a pipe, butt, or puncheon;
And 2 Pipes make a ton. »

Unfortunately, none of these measures had a determinate capacity. Since the gallon which was the unit was of various content, the others varied with it. Moreover, there were varieties of the varieties depending upon the kind of commodity to be measured. JEFFERSON's proposition was to retain the series of measures as quoted above, but to fix the gallon to one determinate capacity and discard the distinction between the wet and dry measure. After considering the different English gallons he chose to propose a gallon of 270 cubic inches, a medium term, and from this to fix the other measures in the series. The measures made for use were to be foursided with rectangular sides and bottoms rather than cylindrical, because of the greater ease in verifying them, and because, as JEFFERSON writes « till the circle can be squared, the cylinder cannot be cubed, nor it's contents exactly expressed in figures ».

For weights, JEFFERSON gives two series, the avoirdupois having the pound of 16 ounces, the ounce of 16 drachms and the drachm of 4 quarters, and the troy series with the pound divided into 12 ounces, the ounce (by the subdivision of the apothecaries) into 8 drachms, the drachm into 3 scruples and the scruple into 20 grains. For gold and silver the ounce is divided into 20 pennyweights and the pennyweight into 24 grains. The weight of the pound troy is to the weight of the pound avoirdupois as 144 to 175. These two series seem at some time to have been interconnected, despite the opinion of the English committees to the contrary. For strangely enough, the proportions between the measures and the weights on deposit made the weight of the dry gallon of wheat equal that of the liquid one of wine, and the avoir-

dupois pound of wheat and the troy pound of wine had the same measure. There was, also, a remarkable correspondence between weights and measures, in that 1000 ounces avoirdupois of pure water filled a cubic foot. For the convenience of the American citizen JEFFERSON proposes to suppress the pound and ounce troy and the drachm and quarter avoirdupois and to join in one series the avoirdupois pound and ounce and the troy pennyweight and grain. He then considers it necessary to refer the weights to a determinate mass of some substance with an invariable specific gravity, for which he choses rain water. He concludes by saying:

« Let it, then, be established that an ounce is of the weight of a cube of rain-water, of one tenth of a foot, or rather, that it is the thousandth part of the weight of a cubic foot of rain-water, weighed in the standard temperature: that the series of weights of the United States shall consist of pounds, ounces, penny-weights, and grains; whereof

24 Grains shall be one penny-weight;
18 Penny-weight one ounce;
16 Ounces one pound. »

38. Concerning the coins JEFFERSON writes:

« Congress in 1786 established the money unit at 375.64 troy grains of pure silver. It is proposed to enlarge this by about the third of a grain, in weight, or a mill, in value; that is to say, to establish it at 376 (or more exactly 376.02985) instead of 375.64 grains, because it will be shewn that this, as the unit of coin, will link in system with the units of length, surface, capacity, and weight, whenever it shall be thought proper to extend the decimal ratio through all these branches. It is to preserve the possibility of doing this, that this very minute alteration is proposed.

We have this proportion, then, 875 to 864, as 376.02985 grains troy to 371.30261, the expression of the unit in the new grains.

Let it be declared, therefore, that the money unit, or dollar of the United States, shall contain 371.3 American grains of pure silver. »

39. The remainder of the report is concerned with a decimal system of weights and measures. JEFFERSON introduces the topic by writing:

« But if it be though that, either now, or at any future time, the citizens of the United States may be induced to undertake a thorough reformation of their whole system of Measures, Weights and Coins, *reducing every branch to the same decimal ratio already established in their Coins, and thus bringing the calculation of the principal affairs of life within the arithmetic of every man who can multiply and divide plain numbers*, greater changes will be necessary. »

(The italics have been added).

In choosing a unit of measure JEFFERSON preferred the second rod to the second pendulum as producing the most coincidencies between the old and the new ratios.

For measures of length JEFFERSON proposes to take the second rod as the unit and divide it into five equal parts, each of which would be called a foot. This foot would, however, be one quarter of an inch shorter than the foot then in use. The series proceeding from this foot is given as follows :

« Let the Foot be divided into 10 inches;
the Inch into 10 lines;
the Line into 10 points;
Let 10 Feet make a decad;
10 Decads a rood;
10 Roods a furlong;
10 Furlongs a mile; ».

Concerning superficial measures JEFFERSON writes :

« Superficial Measures have been estimated, and so may continue to be, in squares of the measures of length, except in the case of lands, which have been estimated by squares, called roods and acres. Let the rood be equal to a square, every side of which is 100 feet. This will be 6.483 English feet less than the English rood every way, and 1311 square feet less in its whole contents, that is to say, about one eighth, in which proportion also 4 rood will be less than the present acre. »

For the unit of capacity JEFFERSON chooses the cubic foot which he calls a bushel, and which would contain 1620.23 cubic inches, English. The series of the measures would then be the bushel of 10 pottles, the pottle of 10 demi-pints, the demi-pint of 10 metres each of which would be a cubic inch; 10 bushels would be called a quarter and 10 quarters a last or double-ton. The measures for use would be foursided, with rectangular sides and bottoms, and the bushel a foot cube, the pottle five inches square and four deep, the demi-pint 2 inches square and 2 1/2 deep, and the metre an inch cube.

The weights would be derived from the weight of a cubic inch of rain water, or the thousandth part of a cubic foot, which would be called an ounce. An ounce would be divided into 10 double-scruples, which in turn would be divided into 10 carats. A carat would contain 10 minims or demi-grains, and a minim 10 mites. Ten ounces would make a pound, 10 pounds a stone 10 stones a kental, and 10 kentals a hogshead.

The change in coinage which JEFFERSON was proposing he had already discussed in the first part of his report.

40. The conclusion of the report is in the nature of a summary, and is as follows :

« Measures, weights and coins thus referred to standards unchangeable in their nature (as is the length of a rod vibrating seconds, and the weight of a definite mass of rain water) will themselves be unchangeable. These standards too are such as to be accessible to all persons, in all times and places. The measures and weights derived from them, fall in so nearly with some of those now in use, as to facilitate their introduction; and, being arranged in decimal ratio, they are within the calculation of everyone who possesses the first elements of arithmetic, and of easy comparison, both for foreigners and citizens, with the measures, weights and coins of other countries. »

JEFFERSON suggests a gradual introduction of the system, but not too long a postponement as that would increase the difficulty of its reception with the increase in population.

41. There is a short appendix to the report containing illustrations and developments of some of the passages. These are chiefly numerical, and the appendix closes with a table giving the measures, weights and coins of the decimal system in terms of those of England then in use in the United States.

42. As we have seen, the report was laid on the table on its reception July 13th. August 12th Congress adjourned to the following December (92). Tuesday, the 7th of December 1790, President WASHINGTON addressed both Houses, and among the business which he presumed would be resumed and which he considered « abundantly urged by their own importance » he listed the establishment of standards of weights and measures (93). Consequently, on December 15th the House of Representatives ordered JEFFERSON's report committed to a Committee of the Whole House (94). December 23rd the Clerk of the House was ordered to inform the Senate of the reception of the report and to present a copy of it (95). This was done the same day and the Senate ordered the communication to lie for consideration (96). The 28th of December the Senate appointed a committee, of

(92) *Journal of the House*, I, 298.
(93) Ibid., 332.
(94) Ibid., 338.
(95) Ibid., 341.
(96) *Journal of the Third Session of the Senate of the United States*, 19-20.

which RALPH IZARD was a member, to consider the report on weights and measures and to report what should be done about it (97).

43. On January 18, 1791 the Speaker laid before the House a letter from the Secretary of State covering a postscript to his former report. These were read and ordered to lie on the table (98). The same happened in the Senate where the letter and enclosure were ordered to lie for consideration (99). The letter addressed to the President of the Senate accompanying the postcript reads :

« I have the honor to enclose you a postscript to the report on measures, weights, and coins, now before your House. This has been rendered necessary by a small arithmetical error detected in the estimate of the cubic foot proposed in that report. The head of superficial measures is also therein somewhat more developed. *Nothing is known, since the last session of Congress, of any further proceedings in Europe on this subject.* » (100)

(The italics have been added).

The *Postscript* to the report makes the following correction :

« In cubing the new foot, which was estimated at .978728 of an English foot, or 11.744736 English inches, an arithmetical error of an unit happened in the fourth column of decimals, and was repeated in another line in the sixth column, so as to make the result one ten thousandth and one millionth of a foot too much. The thousandth part of this error (about one ten millionth of a foot) consequently fell on the metre of measure, the ounce weight, and the unit of money. In the last it made a difference of about the twenty-fifth part of a grain Troy, in weight, or the ninety-third of a cent in value. As it happened, the error was on the favorable side, so that the detection of it approximates our estimate of the new unit exactly that much nearer to the old, and reduces the difference between them to 34, instead of 38 hundredths of a grain Troy; that is to say, the money unit instead of 375.64 Troy grains of pure silver, as established heretofore, will now be 375.98934306 grains, as far as our knowledge of the length of the second pendulum enables us to judge; ... » (101)

The postscript also gives a decimal series of superficial measures, which was not given in the original report.

44. March 1, 1791 RALPH IZARD reported from the committee appointed to consider JEFFERSON's report. The committee's report which was adopted was as follows :

(97) Ibid., 22.

(98) *Journal of the House*, I, 356-357.

(99) *Journal of the Third Session of the Senate of the United States*, 34.

(100) *American State Papers*, Class X, I, 36.

(101) *The Writings of Jefferson*, III, 57-59 : also *American State Papers*, Class X, I, 37.

« That as a proposition has been made to the National Assembly of France, for obtaining a standard of measure, which shall be invariable, and communicable to all nations, and at all times, as a similar proposition has been submitted to the British Parliament, in their last session; as the avowed object of these is, to introduce an uniformity in the measures and weights of the commercial nations; as a coincidence of regulation, by the government of the United States, on so interesting a subject, would be desirable, your Committee are of opinion, that it would not be eligible, at present, to introduce any alteration in the measures and weights which are now used in the United States ». (102)

On October 25, 1791 WASHINGTON, addressing both Houses, said « A uniformity in the weights and measures of the country is among the important objects submitted to you by the constitution; and, if it can be derived from a standard at once invariable and universal, must be no less honorable to the public councils, than conducive to the public convenience » (103). November first the committee of the Senate, appointed the preceding day to consider the necessary business, considered it proper for the Senate to proceed in fixing a standard of weights and measures, and a committee was appointed for that purpose (104).

46. GOUVERNEUR MORRIS wrote to JEFFERSON from Paris on November 7th enclosing « some hints relative to coins, currency, weights and measures ». He said in his letter that the adoption of these on an easy and uniform plan was particularly important to America, continuing :

« Much more so than to most other countries. Every man is called by our constitution to share in the government. A knowledge of statistics is therefore in some measure necessary to every American citizen, and the obtaining of this knowledge will be greatly facilitated by the establishment of a currency, which gives the means of conceiving immediately the value of any sum of foreign money; of a measure, which gives the same means as to distances and the surfaces of countries; and lastly, of a weight, which (combined with the currency) gives at the same time an easy mode for conversion of foreign weights. » (105)

47. One of the points which JEFFERSON wished to emphasize was the advisability of having the money and the weights and measures in the same system. This is well illustrated in a letter

(102) *Journal of the Third Session of the Senate of the United States*, 143 : also *American State Papers*, Class X, I, 38.

(103) *The Writings of Washington*, collected and edited by W. C. FORD, XII, 83 (New York and London 1891).

(104) *Journal of the Senate, being the Fisrt Session of the Second Congress*, 22.

(105) SPARKS, J., *The Life of Gouverneur Morris*, III, 21-22.

to HAMILTON in February 1792, when he wrote : « Should it be thought, however, that Congress may reduce the value of the dollar, I should be for adopting for our unit, instead of the dollar, either one ounce of pure silver, or one ounce of standard silver, so as to keep the unit of money a part of the system of measures, weights and coins » (106). This would have been a greater change, though with a similar purpose, than the adoption of the unit as 375.98934306 grains of pure silver as stated in the postscript to his report.

48. April 5, 1792 Senator RALPH IZARD made the report of the committee appointed by the Senate to consider the subject of weights and measures. The opinion of the committee was given in six points as follows : (1) The standard for the weights and measures of the United States should be the second rod, as we have seen it defined above in JEFFERSON's report. (2) The President was to be requested to have such a rod provided, made as carefully as possible, either under the parallel of 45° or in a more convenient place and then reduced to 45°. (3) The expense of the measures should be defrayed by the public. (4) The rod was to be divided as proposed in the last part of JEFFERSON's report. (5) The superficial measures were suggested exactly as JEFFERSON had proposed them in his report, divided as he had added in his postscript. (6) The measures of capacity proposed were likewise those set forth by JEFFERSON. (7) The same can be said of the weights. Thus this report was but a condensation of the second alternative of JEFFERSON's report. The consideration of it was postponed until the next session of Congress (107).

49. November 21st JAMES MONROE wrote to JEFFERSON discussing the report on weights and measures (108).

50. December 6th the Senate had the report of April 5th recommitted and requested the committee to report a bill making the weights and measures uniform and invariable, retaining in general the weights and measures then in use (109). December 17th the Senate resumed consideration of the report of April 5th

(106) *The Writings of Jefferson*, VIII, 298-299.

(107) *Journal of the Senate, being the First Session of the Second Congress*, 173-174 ; also *American State Papers*, Class X, I, 48, giving the date April 4, 1792.

(108) *Bureau of Rolls and Library, Bulletin* 8, 402.

(109) *Journal of the Senate, being the Second Session of the Second Congress*, 18.

together with the motion of December 6th. A motion was made to postpone consideration thereof and to « *Resolve*, That the present measures of length be retained and fixed by an invariable standard; that the measures of surface remain as they are, and be invariable also as the measures of length to which they are to refer; that the unit of capacity now so equivocal, be settled at a medium and convenient term, and defined by the same invariable measures of length, that the more known terms in the two kinds of weights be retained, and reduced to one series, and that they be referred to a definite mass of some substance, the specific gravity of which never changes; and that acommittee [sic] be appointed to bring in a bill accordingly— » The consideration of the motion was deferred until the next day (110).

Consequently, the next day, Tuesday December 18, 1792, the Senate resumed consideration of the motion to postpone consideration of the report of April 5th, with the motion of the 6th of December. They amended the motion of postponement of the previous day by leaving out the appointment of a committee to bring in a bill. They postponed the motion of postponement and took up a motion keeping the weights and measures equal to those then in use. The standard was to be the second rod. The unit of measure was to be a foot equal in length to one hundred parts of the standard rod. Sixty-six feet were to be a chain, and eighty chains a mile. The measures of surface were to be made by squares of the measures of length, but the unit for land was to be an acre equal to 43,560 square feet, or ten chains in length and one in breadth. The unit of the measure of capacity was to be a bushel, one foot square and one and twenty-five cents of a foot deep, containing one cubic foot and a quarter. The unit of weights was to be a pound equal to the pound avoirdupois then in use, and equal in weight to a quantity of rain water, 20 cents of a foot square and 40 cents deep, or 16000 cubic cents of a foot, measured and weighed in a cellar of uniform natural temperature. The units were to be divided into hundredth parts and where necessary into thousandths or mills and in the case of weights the mill was to be divided into seven grains equal to seven grains of troy weight. This motion together with that of postponement

(110) Ibid., 22.

was then referred to a committee, of which JOHN RUTHERFORD was a member (111).

51. December 25th JEFFERSON wrote to RUTHERFORD (112) on these two motions before the Senate, commenting on them, suggesting minor changes in them, and giving reasons for his stand in the original report. The letter is given in extenso in the Appendix (number V) to this paper.

52. January 8, 1795 the President sent a written message to both Houses in which he wrote that the minister of the French Republic had communicated to the Secretary of State certain proceedings of the Committee of Public Safety relating to weights and measures, which the President laid before Congress (113). The following day the Senate ordered three hundred copies of the French decree of the Committee of Public Safety, referred to by the President, printed for its use (114).

The decrees of this committee were of the end of the year 1793. The unit of measure was taken as the ten millionth part of the 4th part of a terrestrial meridian, and that of weight was equal to the weight of a cubic measure of distilled water having its side equal to the tenth part of the measure, weighed in open air, in the temperature of melting ice. These measures were divided into decimal parts, the system of division to be adopted in all measures, including that of the circle. The terrestrial arc contained between Dunkirk and Barcelona was being measured for fixing the details of the system (115).

53. The House of Representatives on December 24, 1795 appointed a committee to examine and report on so much of JEFFERSON's report of 1790 and so much of the President's message of January 8, 1795 as related to weights and measures (116). April 12, 1796 HARRISON reported for this committee. His report was read and ordered committed to a Committee of the Whole House (117). It gave three principles for regulating the standards

(111) Ibid., 22-23.

(112) *The Writings of Jefferson*, IX, 1-6.

(113) *Journal of the Senate of the United States of America*, II, 142 (Washington 1820, 1821): also *Journal of the House*, II, 288.

(114) *Journal of the Senate*, II, 143.

(115) *American State Papers*, Class X, I, 115-116.

(116) *Journal of the House*, II, 388.

(117) Ibid., 510.

of weights and measures, namely (1) that measures of surface, capacity, and weight should be regulated by length; (2) that the units adopted ought not to vary greatly from the foot and the pound avoirdupois then in use; and (3) that it was worthy of the attention of the Legislature to refer to some measure of length derived from a constant in nature. The report suggested methods of division for the pound (1) in a decimal ratio to one thousand parts, and these into seven parts each; (2) in a decimal ratio to the ten thousandth part; (3) into 16 ounces and the ounce into 18 parts, each of which to be divided into 24; (4) the division of the ounce in a decimal ratio to the one thousandth part. The committee recommended further experiment with the second rod and with the weight of rain-water, and suggested ascertaining the weights of the divisions of the ounce and pound (118).

May 14th a committee of the Whole House considered the report of April 12th, and two resolutions were twice read and agreed to by the House. They were (1) that the President be authorized to employ persons who should make experiments on the second rod and on the weight of a given quantity of rain water and ascertain the weights of the divisions of the pound and the ounce as given in HARRISON's report; and (2) that a sum no greater than 1000 dollars be appropriated to defray the expenses of the experiments. It was ordered that a bill or bills pursuant to the resolutions be prepared and submitted (119). Such a bill was submitted by HARRISON on May 16th and was twice read and committed to a Committee of the Whole House (120). May 18th it was thus considered, and ordered engrossed (121). May 19th the engrossed bill was read for the third time and passed. It was given the title « An act directing certain experiments to be made to ascertain uniform standards of weights and measures for the United States ». It was ordered that the Clerk carry the bill to the Senate (122). That day it was presented and read in the Senate (123). May 20th it was read there for

(118) *American State Papers*, Class X, I, 148-149.
(119) *Journal of the House*, II, 557.
(120) Ibid., 559.
(121) Ibid., 562.
(122) Ibid., 563.
(123) *Journal of the Senate*, II, 265.

the second time and referred to a committee (124). This committee reported on May 31st, and their report was ordered printed and the consideration of the bill was deferred to the next session. The Secretary of the Senate was ordered to acquaint the House of Representatives therewith (125). It does not seem that the bill was ever voted against. It seems that it merely fell by the wayside.

54. The decimal system was being much discussed about this time. CHAUNCEY LEE (126) in his *American accomptant* went so far as to make the statement, « Indeed, in my humble opinion, vulgar fractions are a very unimportant, if not useless part of Arithmetic, and decimals only of any considerable practical consequence » (127). Then he proceeded to propose a decimal system of weights and measures, which, however, lacked unity, so that different commodities were measured by different systems.

55. January 12, 1798 the House of Representatives again took up the subject of weights and measures and appointed a committee to prepare a plan fixing them (128). This order seems to have had no result. February 28, 1804 a communication reached the Senate, that the Commonwealth of Pennsylvania in the House of Representatives had urged Congress to fix the standard of weights and measures throughout the United States (129). March 2nd 1804 the House of Representatives appointed a committee to inquire into the expediency of fixing the standard of weights and measures (130). This measure, too, seems to have had no result.

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(124) Ibid., II, 267.

(125) Ibid., 283-284.

(126) CHAUNCEY LEE (1763-1842), the son of the Reverend JONATHAN LEE, was born in Salisbury, Litchfield County, Connecticut, on November 9, 1763. He was graduated from Yale in the Class of 1784 and then studied law, being admitted to the bar in 1787. He later turned to the ministry and was licensed to preach June 3, 1789. In 1823 he received the degree of Doctor of Divinity from Columbia College. Aside from *The American accomptant* most of his writings are religious. For further information concerning LEE, see FRANKLIN BOWDITCH DEXTER, *Biographical Sketches of the Graduates of Yale College*, IV, 351-355 (New York 1907).

(127) LEE, CHAUNCEY, *An American accomptant*, xiv (Lansingburgh 1797).

(128) *Journal of the House*, III, 132.

(129) *American State Papers*, Class X, I, 388.

(130) *Journal of the House*, IV, 611.

56. December 20, 1807 JOHN DORSEY wrote to JEFFERSON on the subject of weights and measures (131). This letter JEFFERSON acknowledged January 21, 1808 in the letter already quoted (§ 29.), making a few suggestions on the proposed plan. DORSEY again wrote to him on the same subject on March 26th (132). That same year he received notes on DORSEY's plan from ALBERT GALLATIN (133).

57. January 7th of the same year, 1808, two resolutions of the Legislature of the State of New Jersey, relative to uniformity of weights and measures, were read to the House of Representatives. The House appointed a committee to inquire into the propriety of establishing a standard by law to fix the weights and measures (134). That effort was just as fruitless as the preceding ones.

58. Although action by Congress on weights and measures grew less and less, the people were still alive to the need of some reformation. JEFFERSON's interest did not die down, and he kept up a steady correspondence on the subject with various people. October 27, 1808 he wrote a particularly interesting letter to THOMAS COOPER, acknowledging a letter of the 16th and enclosing a copy of his report of 1790 (135). The letter speaks of JEFFERSON's severe illness at the time of composing the report, as causing the error in calculating the money unit, which was corrected in the postscript of 1791. JEFFERSON writes that it is first necessary to settle the question whether the units of measures, weights and coins should have a known relation to something fixed in nature, or whether an arbitrary standard should be chosen. He says that DORSEY is in favor of the aribitrary standard of one third of the standard yard of H. G. of England, but that he himself prefers a unit bearing a given relation to a constant in nature. Of the decimal system he writes:

«I have lately had a proof how familiar this division into dimes, cents, and mills, is to the people when transferred from their money to any-thing else. I have an odometer fixed to my carriage, which gives the distances in miles, dimes

(131) *Bureau of Rolls, Bulletin* 8, 157.
(132) Ibid., 158.
(133) Ibid., 255.
(134) *Journal of the House*, VI, 113.
(135) *The Writings of Jefferson*, XII, 180-182.

and cents. The people on the road inquire with curiosity what exact distance I have found from such a place to such a place; I answer, so many miles, so many cents. I find they universally and at once form a perfect idea of the relation of the cent to the mile as an unit. »

JEFFERSON concludes by saying that he is open to compromise as well as to other plans of reformation.

59. JEFFERSON's letter of September 11, 1811 (136) to ROBERT PATTERSON, Director of the Mint and member of the American Philosophical Society of which JEFFERSON was then President, has already been mentioned in connection with the latter's disapproval of the French metric system (§ 29.). In this letter he further writes :

« Why should not the college of the literary societies of the world adopt the second pendulum as the unit of measure on the authorities of reason, convenience and common consent? And why should not our society open the proposition by a circular letter to the other learned institutions of the earth? If men of science, in their publications, would express measures always in multiples and decimals of the pendulum, ... , they would soon become familiar to all men of instruction, and prepare the way for legal adoptions. »

The letter goes on to speak of the attempt of the French to introduce a decimal division of the circle. The advantages of this JEFFERSON finds would not then compensate for the embarrassments of a change.

A month later, November 10, 1811, JEFFERSON again wrote to PATTERSON, as was shown above (§ 29.) (137). This letter acknowledges one from PATTERSON of the 23rd of September and then proceeds to give JEFFERSON's ideas on the subject of weights and measures requested by the Philosophical Society, which had just concluded its consideration of the subject. The first part is a summarization of the ideas previously expressed and is followed by a discussion of JEFFERSON's proposal the previous month that the scientific societies band together in establishing a standard and a system. It is a long letter and JEFFERSON's plan for offering his metrical system to the world is a complicated one. It was to be accomplished by a series of circular letters from one learned society to another accompanied by tables of the standards in use in the countries of the societies, expressed in terms of the new system. Care would have to be taken not

(136) Ibid., XIII, 85-89.
(137) Ibid., 95-108.

to hurt the feelings of any country. Thus England would be asked to collaborate with the United States, since the two countries were using the same system. However, the plan bore no fruit. The letter was acknowledged in one from PATTERSON dated January 10, 1812 in which he gave his observations on the standard of weights and measures (138). JEFFERSON also wrote to PATTERSON again on the subject, his letter being dated May 29, 1812 (139).

60. We have already spoken of JEFFERSON's letter to JOHN QUINCY ADAMS (§ 30.) on November 1, 1817, in response to a letter from the latter dated October 4th (140) asking for information concerning weights and measures upon which he was preparing a report. The letter contains nothing not already stated in other of his papers. ADAMS' report, in conformity to a resolution of the Senate of March 3rd, 1817, was not submitted until February 22, 1821 (141).

61. It is truly a pitty that through negligence and inertia the important quesion of a decimal system of weights and measures, a system based on the same principles as our numbers, came to naught, for, as JEFFERSON wrote in his *Autobiography*, « The divisions into dimes, cents, and mills is now so well understood, that it would be easy of introduction into the kindred branches of weights and measures » (142).

APPENDICES

Appendix I Manuscripts in the Library of the Department of State.

The following manuscripts would no doubt prove very helpful in verifying coinage and mint reports, but unfortunately they are not available to the author of this paper at the present time. It seems that they have never been published but are listed under the title « Catalogue of the Papers of the Continental Congress » in the *Bulletin of the Bureau of Rolls and Library of the Department of State*, No. 1, September 1893. They may be consulted in that library with the permission of the Secretary of State.

No. 26. Reports of the committees on treasury and finance, from 1776 to 1788. In 1 volume, folio.

No. 87. Official letters from THOMAS JEFFERSON, Minister Plenipotentiary to the Court of France, to Congress and to the Secretary of Foreign Affairs

(138) *Bureau of Rolls*, *Bulletin* 8, 443.
(139) Ibid., *Bulletin* 6, 366.
(140) Ibid., *Bulletin* 8, 15.
(141) Adams, J. Q., *op. cit.*
(142) *The Writings of Jefferson*, I, 79.

(JOHN JAY). (Most of the correspondence of 1787 is missing.) In 2 volumes, folio.

No. 107. Transcript of the letters of THOMAS JEFFERSON, from May 11, 1785, to August 6, 1787. In 2 volumes, folio.

No. 136. Reports of the Board of Treasury from 1776 to 1781. Arranged chronologically in 5 volumes, folio.

No. 137. Letters and reports of ROBERT MORRIS, Superintendent of Finance and Agent of Marine, from 1781 to 1784. In 3 volumes, folio, with an appendix.

No. 186. « A Committee Book; » memoranda of dates of reports of committees, 1782 to 1785.

No. 188. « A record of the Reports of Committees of Congress on the five executive departments, 1782-1785. » (Unfinished), folio.

No. 189. « Lists of Reports of Committees [of Congress] from 1786 to 1788. »

No. 190. « Lists of Reports of Committees [of Congress] from 1785 to 1788. »

Appendix II Resolutions (143) *proposed by* JEFFERSON *in his « Notes on the Establishment of a Money Unit, and of a Coinage for the United States », 1783.*

« ... Suppose they [the Committee of the States] be instructed,

To appoint proper persons to assay and examine, with the utmost accuracy practicable, the Spanish milled dollars of different dates, in circulation with us.

To assay and examine, in like manner, the fineness of all the other coins which may be found in circulation within these States.

To report to the Committee the result of these assays, by them to be laid before Congress.

To appoint, also, proper persons to enquire what are the proportions between the values of fine gold, and fine silver, at the markets of the several countries with which we are, or probably may be, connected in commerce; and what would be a proper proportion here, having regard to the average of their values at those markets, and to other circumstances, and to report the same to the Committee, by them to be laid before Congress.

To prepare an Ordinance for establishing the Unit of Money within these States; for subdividing it; and for striking coins of gold, silver, and copper, on the following principles :

That the Money Unit of these States shall be equal in value to a Spanish milled dollar containing so much fine silver as the assay, before directed, shall show to be contained, on an average, in dollars of the several dates in circulation with us.

That this Unit shall be divided into tenths and hundredths; that there shall be a coin of silver of the value of a Unit; one other of the same metal, of the value of one-tenth of a Unit; one other of copper, of the value of the hundredth of a Unit.

That there shall be a coin of gold of the value of ten Units, according to the report before directed, and the judgment of the Committee thereon.

That the alloy of the said coins of gold and silver, shall be equal in weight to one-eleventh part of the fine metal.

That there be proper devices for these coins.

That measures be proposed for preventing their diminution, and also their currency, and that of any others, when diminished.

That the several foreign coins be described and classed in the said Ordinance,

(143) Ibid., 250-251.

the fineness of each class stated, and its value by weight estimated in Units and decimal parts of Units.

And that the said draught of an Ordinance be reported to Congress at their next meeting, for their consideration and determination.»

Appendix III JEFFERSON'*s letter to* DAVID RITTENHOUSE,

New York, June 12, 1790 (144)

Dear Sir,—I know not what apology to make for the trouble I am about to give you. I am sure I must call your public spirit in aid of your private friendship to me.

You will see by the head of the enclosed report that the House of Representatives have instructed me to lay before them a plan for establishing uniform weights, measures and coins. Five and twenty years ago I should have undertaken such a task with pleasure because the sciences on which it rests were then familiar to my mind and the delight of it. But taken from them through so great a length of time and forced by circumstances into contemplations of a very different nature and much less pleasing, I have grown rusty in my former duties. To render me more unequal to the task it comes on me in the moment when I am separated from all my books and papers, which have been left in Paris and Virginia; and this place yields fewer resources in the way of books than could have been imagined. Thus situated I have done what I could toward fulfilling the object of the House of Representatives and I think myself happy in having such a resource as your friendship and your learning to correct what I have prepared for them. The necessity I am under must be my apology. It is desirable for the public that the plan should be free from errors; it is desirable for me that they should be corrected privately by a friend rather than before the world at large by the unfeeling hand of criticism.

Do then, my dear Sir, read it over with all that attention of which you are so much the master and correct with severity everything you find in it which is not mathematically just, and while I ask your attention to every part of it I will make some notes on particular things. It was not till I had got through the work that I was able to get a sight of Whitehurst's on the same sucject. Mr. Madison procured it for me from a library in Philadelphia, but on perusal of it his plan of two pendulums appeared to me on the whole less eligible than Leslie's of the vibrating rod. How has it been inferred that Sir Isaac [Newton's calculation of the length of the pendulum for the latitude of London is 39.2 inches? From his general table in the Principia, book 3, I deduce 39.1682. Am I wrong? If so, I may be equally wrong in the calculation of 39.1285 miles [sic] for latitude 38°, in which an error would be fundamentally important, and, therefore, I ask your particular attention to that. What do you think of the vibrating rod? Is it not clear of some objections which lie against the pendulum? and is it liable to any of which that is clear? and which is the best on the whole? I am too little familiar with the mechanism of the clock to know how the rod can be adapted to a machinery which shall maintain and count its vibrations, without accelerating them by its power or retarding them by its friction, in short, which shall leave the rod free to make its vibrations uninfluenced by any circumstance but its own length. On this point no man in the world can judge or contrive better than yourself. On this, then, and all the other points of the report, pray give me the full benefit of your assistance. In the trouble I am giving you I feel it

(144) Ibid., XIX, 73-75.

as a circumstance of additional misfortune that I am pressed in time. The session of Congress is drawing to a close. Nothing will keep them together after the money bills are got through, and this will be pretty soon. I am obliged, therefore, to ask your immediate attention to this subject as I must give in the report before they rise. I suppose they will have it printed for consideration to be taken up at their next meeting.

I will not weary you with apologies though I feel powerfully the necessity of them. I am, in all times and places, your sincere friend and humble servant.

Appendix IV JEFFERSON's *letter to* DAVID RITTENHOUSE.

New York, June 20, 1790. (145)

Dear Sir,—I enclosed you on the 17th the alterations I had made in my report in consequence of the Bishop of Autun's proposition, which had come to my hands two days before. On the 18th, I received from Mr. Cutter in London a packet of newspapers, among which were the two enclosed, containing the speech in Parliament of Sir John Riggs Miller, on the subject of weights and measures. I observe, he states the estimate of 39.2 I. for the length of the pendulum as confessedly erroneous. I had adopted it from memory only, and before I had been able to get a single book of any kind, in the first part of the report, wherein I endeavor to ascertain and fix invariably the system of measures and weights now in use with us. But before I proceeded to the second part, proposing a thorough reform, and reducing the whole to the decimal ratio, I had been able to procure here a copy of the Principia, and so to recur to the fountain head for Sir I. Newton's calculations, and then added the note which you will find page 3 of the report, doubting what could have been the foundation of the common imputation of the estimate of 39.2 to Sir I. Newton, and stating the grounds of that of 39.1682 for the latitude of 51° 31' of 39.1285 for 38°, which I had at first adopted, and 39.14912 for 45°, which I took on receiving the Bishop of Autun's proposition. I have now thought I might venture to take for granted, that the estimate of 39.2 is as erroneous as I had supposed it, and therefore to expunge it from the first branch of the report, and substitute in its stead 39.1682; and to change a passage under the head of « Measures of length » into the following form :

« They furnish no means to persons at a distance of knowing what this standard is. This, however, is supplied by the evidence of the second pendulum, which, according to the authority before quoted, being 39.1682 I. for the latitude of London, and consequently the second rod for the same latitude being 58.7523, we are first to find by actual trial the rod for 45°, and to add to that 287/10000 of an inch, or rather 3/10 of a line (which in practice will endanger less error than an attempt at so minute a fraction as 10,000th parts of an inch), this will give us the true measure of 58 3/4 English inches. Or, to shorten the operation, and yet obtain the result we seek, let the standard rod of 45° be divided into 587 1/5 equal parts, and let each of these parts be declared a line, and ten lines an inch, » etc.

I propose also to strike out the note (page 3) before mentioned, and to substitute the following in its place :

« The length of the pendulum has been differently estimated by different persons. Knowing no reason to respect any of them more than Sir Isaac Newton for skill, care, or candor, I had adopted his estimate of 39.149 I. for our northern limit of 45°, before I saw the different propositions of the Bishop of Autun, and

(145) Ibid., VIII, 38-41.

Sir John Riggs Miller. The first of these gentlemen quotes Mairan's calculation for 48° 50', the latitude of Paris, to wit, 504:257::72:to a 4th proportional, which will be 36.71428 = 39.1923 inches. The difference between the pendulum for 48° 50' and 45°, as calculated by Sir I. Newton, is .0112 I. : so that the pendulum for 45° would be estimated, according to the Bishop of Autun, at 39.1923 — .0112 = 39.1811. Sir John Riggs Miller proposes 39.126, being Graham's determination for 51° 31', the latitude of London. The difference between the pendulum for 51° 31', and 45° by Sir I. Newton, is .019 I., so that the pendulum for 45° should be estimated according to Sir J. R. Miller, at 39.126 — .019 = 39.107 I. Now, dividing our respect between these two results, by taking their mean, to wit, $\frac{39.181 + 39.107}{2} = 39.144$, we find ourselves almost exactly with Sir I. Newton, whose estimate of 39.149 we had already adopted. »

I propose also to reform a passage under the head of Weights, in the first branch of the report, to stand thus :

« Let it then be established, that an ounce is the weight of a cube of rain water of one-tenth of a foot, *or rather, that it is the thousandth part of the weight of a cubic foot of rain water* weighed in the standard temperature, » etc.

All which I submit to your judgment, and I will ask you particularly to examine the numbers .0112 and .019, as I have no help here to find them otherwise than by approximation. I have wished much, but in vain, Emerson's and Ferguson's books here. In short, I never was cut off from the resources of my own books and papers at so unlucky a moment. There is a Count Andriani, of Milan, here, who says there is a work on the subject of weights and measures published by Trisi of Milan. Perhaps you may have it at Philadelphia, and be able to send it to me. Were it not for my confidence in your assistance, I should not have ventured to take up this business till I received my books.

I am, my dear Sir, with great and sincere esteem, your friend and servant.

Appendix V JEFFERSON'*s letter to* JOHN RUTHERFORD.

Philadelphia, December 25, 1792. (146)

Sir,—I have considered, with all the attention which the shortness of the time would permit, the two motions which you were pleased to put into my hands yesterday afternoon, on the subject of weights and measures, now under reference to a committee of the Senate, and will take the liberty of making a few observations thereon.

The first, I presume, is intended as a basis for the adoption of that alternative of the report on measures and weights, which proposed retaining the present system, and fixing its several parts by a reference to a rod vibrating seconds, under the circumstances therein explained; and to fulfil its object, I think the resolutions there proposed should be followed by this : « that the standard by which the said measures of length, surface, and capacity shall be fixed, shall be an uniform cylindrical rod of iron, of such length as in latitude forty-five degrees, in the level of the ocean, and in a cellar or other place of uniform natural temperature, shall perform its vibrations in small and equal arcs, in one second of mean time; and that rain water be the substance, to some definite mass of which, the said weights shall be referred. » Without this, the committee employed to prepare a bill on those resolutions, would be uninstructed as to the principles by which the Senate

(146) Ibid., IX, 1-6.

mean to fix their measures of length, and the substance by which they will fix their weights.

The second motion is a middle proposition between the first and the last alternatives in the report. It agrees with the first in some of the present measures and weights, and with the last, in compounding and dividing them decimally. If this should be thought best, I take the liberty of proposing the following alterations of these resolutions :

2d. For « metal » substitute « iron ». The object is to have one determinate standard. But the different metals having different degrees of expansibility, there would be as many different standards as there are metals, were that generic term to be used. A specific one seems preferable, and « iron » the best, because the least variable by expansion.

3d. I should think it better to omit the chain of 66 feet, because it introduces a series which is not decimal, viz., 1.66.80. and because it is absolutely useless. As a measure of length, it is unknown to the mass of our citizens; and if retained for the purpose of superficial measure, the foot will supply its place, and fix the acre as in the fourth resolution.

4th. For the same reason, I propose to omit the words « or shall be ten chains in length and one in breadth. »

5th. This resolution would stand better, if it omitted the words « shall be one foot square, and one foot and twenty cents of a foot deep, and, » because the second description is perfect, and too plain to need explanation. Or if the first expression be preferred, the second may be omitted, as perfectly tautologous.

6th. I propose to leave out the words « shall be equal to the pound avoirdupois now in use, and, » for the reasons suggested in the second resolution, to wit, that our object is, to have one determinate standard. The pound avoirdupois now in use is an indefinite thing. The committee of parliament reported variations among the standard weights of the exchequer. Different persons weighing the cubic foot of water, have made it, some more, and some less than one thousand ounces avoirdupois; according as their weights had been tested by the lighter or heavier standard weights of the exchequer. If the pound now in use be declared a standard, as well as the weight of sixteen thousand cubic cents of a foot in water, it may hereafter perhaps be insisted that these two definitions are different, and that, being of equal authority, either may be used, and so the standard pound be rendered as uncertain as at present.

7th. For the same reason, I propose to omit the words « equal to seven grains troy. » The true ratio between the avoirdupois and troy weights, is a very contested one. The equation of seven thousand grains troy to the pound avoirdupois, is only one of the several opinions, and is indebted perhaps to its integral form for its prevalence. The introduction either of the troy or avoirdupois weight into the definition of our unit, will throw that unit under the uncertainties now enveloping the troy and avoirdupois weights.

When the House of Representatives were pleased to refer to me the subject of weights and measures, I was uninformed as to the hypothesis on which I was to take it up; to wit, whether on that, that our citizens would not approve of any material change in the present system, or on the other, that they were ripe for a complete reformation. I therefore proposed plans for each alternative. In contemplating these, I had occasion to examine well all the middle ground between the two, and among others which presented themselves to my mind, was the plan of establishing one of the known weights and measures as the unit in each class; to wit, in the measures of lines, of surfaces, and of solids, and in

weights, and to compound and divide them decimally. In the measure of weights, I had thought of the ounce as the best unit, because, calling it the thousandth part of a cubic foot of water, it fell into the decimal series, formed a happy link of connection with the system of measures on the one side, and of coins on the other, by admitting an equality with the dollar, without changing the value of that or its alloy materially. But on the whole, I abandoned this middle proposition, on the supposition that if our fellow citizens were ripe for advancing so great a length towards reformation, as to retain only four known points of the very numerous series to which they were habituated, to wit, the foot, the acre, the bushel, and the ounce, abandoning all the multiples and subdivisions of them, or recurring for their value to the tables which would be formed, they would probably be ripe for taking the whole step, giving up these four points also, and making the reformation complete; and the rather, as in the present series and the one to be proposed, there would be so many points of very near approximation, as aided in the same manner by tables, would not increase their difficulties perhaps, indeed, would lessen them by the greater simplicity of the links by which the several members of the system are connected together. Perhaps, however, I was wrong in this supposition. The representatives of the people in Congress are alone competent to judge of the general disposition of the people, and to what precise point of reformation they are ready to go. On this, therefore, I do not presume to give an opinion, nor to pronounce between the comparative expediency of the three propositions: but shall be ready to give whatever aid I can to any of them which shall be adopted by the Legislature.

I have the honor to be, with perfect respect, your most obedient, and most humble servant.

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New York

C. Doris Hellman.

The discovery of the mammalian egg and the foundation of modern embryology

With a complete facsimile (no. XIII) of K. E. VON BAER's *fundamental memoir : De ovi mammalium et hominis genesi (Leipzig,* 1827).

1. Early embryology, 2. v. BAER's immediate predecessors, 3. His own discovery, 4. Publication of the *Epistola*, 1827, 5. Analysis of the *Epistola*, 6. Reception of his discovery, 7. His later work, 8. His colleagues. The Baltic founders of modern embryology, 9. His life, 10. Bibliography, 11. Iconography, 12. Thanks.

Facsimile no. XIII: *De ovi mammalium et hominis genesi* (Leipzig, 1827).

1. **Early embryology.** — Though the embryological knowledge taught in our medical schools is relatively recent, a complete history of embryology would take us back to the earliest times. Indeed we may assume that every thinking man has asked himself some questions with regard to the formation and development of embryos, for such questions are continually forced upon him by life itself. Any attempt to answer them, however crude, was a contribution to embryology, and the elaborate treatises available to modern students have been gradually produced by the constant accumulation, selection, improvement, arrangement, and rearrangement of the answers.

The medical schools of Cos, Cnidos, and of Sicily showed already much interest in the subject, and the earliest systematic treatment of it may be found in a group of Hippocratic treatises which are probably of Cnidian origin and date from the first quarter of the fourth century B. C. (1) Somewhat later appeared

(1) SARTON, *Introduction* (vol. 1, 119-21, etc.).

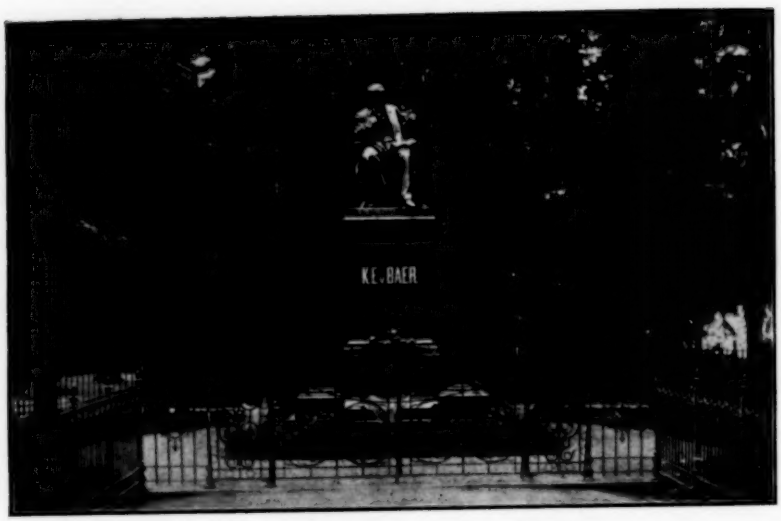
ARISTOTLE's treatise *De generatione animalium*, then later still various writings of the Alexandrian school, finally those of GALEN, which were the culmination of more than six centuries of scientific efforts. To make a detailed examination of embryological ideas down to the seventeenth century would take us too far away. It will suffice to say that the mediaeval thoughts on the subject were dominated, directly or indirectly, by the writings of ARISTOTLE and GALEN.

In the seventeenth century new and fundamental investigations were made by HIERONYMUS FABRICIUS AB AQUAPENDENTE (*De formato foetu*, c. 1604; etc.), by WILLIAM HARVEY (*Exercitationes de generatione animalium*, London, 1651), and by MARCELLO MALPIGHI (*De formatione pulli in ovo*, London, 1673; etc.). MALPIGHI's papers containing many careful observations, splendidly illustrated, seemed to open a very promising field, but unfortunately his investigations and those of contemporary microscopists, notably SWAMMERDAM and LEEUWENHOEK, were soon thrown into the background by unprofitable discussions. At least they seem to have been unprofitable; they were certainly premature.

These discussions were of two kinds. First in chronological order and also in importance, there was the conflict between epigenesists and « evolutionists ». The former claimed that the development of the egg or of the embryo was really a new development, a new creation or construction—like that of a house by means of undifferentiated stones. This was really the Aristotelian and the Harveian point of view. According to the « evolutionists » on the contrary, the embryological development was simply an unfolding, an evolution (hence their name), comparable to that of the bud opening out into a flower. This theory of pre-formation (or pre-delineation) received, curiously enough, the support of two of the best biological observers of that time (and indeed of all times), MALPIGHI and SWAMMERDAM, and there was in it something mystical which made a strong appeal to the more philosophically minded among the naturalists, ALBRECHT VON HALLER and CHARLES BONNET, not to speak of philosophers, such as LEIBNIZ. Under philosophical patronage these ideas were soon carried to their logical conclusion, the absurd theory of the incasement (emboitement) of germs. This implied for example —as SWAMMERDAM's eccentric genius had already seen it—that

KARL ERNST von BAER

Isis, XVI, pl. 5. (1792-1876) G. SARTON.



Monument v. BAER (Dorpat)

Isis, XVI, pl. 6.

G. SARTON.

mother Eve carried the whole of mankind neatly folded in her womb.

The other discussion was started by the sensational discovery of spermatozoa which LEEUWENHOEK published in 1678. According to him and his followers,—the animalculists (or spermists), —the sperm was the real germ, the female egg being only a sort of nest wherein this germ was hatched. Their adversaries, called ovists (or ovulists), claimed that the egg was the true germ and that the sperm was comparatively insignificant.

Thus was the fine observational tradition of the seventeenth century interrupted, or at any rate considerably slowed down for more than a century by discussions which were irrelevant, because they were too far ahead of the experimental data. The only work of the eighteenth century which continued the efforts of FABRICUS, HARVEY, and MALPIGHI and deserves to be quoted together with theirs was that of CASPAR FRIEDRICH WOLFF (*Theoria generationis*, Halle, 1759; *De formatione intestinorum*, St. Petersburg, 1768-69). WOLFF's investigations were very remarkable, and they earned VON BAER's unstinted admiration; in some respects they were almost prophetic, for example we find in them an adumbration of the theory of germ layers; yet his actual observations were hardly on the high level which MALPIGHI had attained almost a century before.

2. **Baer's immediate predecessors.** — So much for the distant background, but as we come nearer to the discovery to which this paper is devoted, we want to see the stage more clearly. A good account of the antecedents of his discovery was given by V. BAER himself in his autobiography (1866, 306-26), and more briefly in the preface to the memoir reproduced below (p. I-V). I have checked the main points of it to my satisfaction. V. BAER begins by explaining the unsuccessful experiments made by HALLER, in 1752, on some forty sheep, and later, on other animals. As HALLER failed to recognize anything like an egg except in a relatively late stage (after a fortnight), he concluded that only a fluid substance was carried to the uterus, and that after a long time out of that fluid « curdled » (gerinnen) not only the chorion but the embryo itself. This may seem fantastic, but consider HALLER's evidence; he found no trace of an egg

anywhere if the dissection was made too soon after the ewes had been covered, but if one waited a little longer a small embryo was discovered in the womb. Thanks to HALLER's immense authority his conclusion was generally accepted, and it was taught in the medical schools until the very time of V. BAER's discovery; V. BAER himself learned it in Dorpat. (2)

Yet glimpses of the truth had been already seen by at least three men, duly mentioned by V. BAER. In 1797 (1778) (3) WILLIAM CUMBERLAND CRUIKSHANK of Edinburgh had found ova, considerably smaller than the Graafian vesicles, in the oviducts of a rabbit three days after impregnation, and on the fourth day, he found them in the uterus. These experiments (29 in number) were very remarkable indeed, and, being published in one of the leading scientific journals of the time, it is strange that they did not attract more attention. The only explanation is that the prejudice against the concept of mammalian egg, a prejudice confirmed by HALLER's authority, was then too strong to be shaken. (4)

CRUIKSHANK's observations were repeated by JEAN LOUIS PRÉVOST (1790-1850) and the future chemist, JEAN BAPTISTE DUMAS (1800-84) and published in their memoir *De la génération dans les mammifères et des premiers indices du développement de l'embryon* (*Annales des sciences naturelles*, vol. 3, 113-38, 3 pl., Paris, 1824).

Finally, another approach to the complete discovery was made in 1825 by the great Czech biologist, JOHN EVANGELISTA PURKINJE (1787-1869), a man whose versatile genius has not yet received

(2) CARL FRIEDRICH BURDACH, *Die Physiologie als Erfahrungswissenschaft*. Mit Beiträgen von K. E. v. BAER und HEINRICH RATHKE (6 vols., Leipzig, 1826-40; vol. 1, 75).

(3) CRUIKSHANK's experiments were made at Chelsea in 1778 at HUNTER's expense, but were not « read » until 1797 (*Philosophical Transactions*, vol. for 1797, 197-214, 1 pl.)

(4) HALLER had expressed himself wery strongly. « Denique, quod caput rei est, neque HARTMANNUS cum experimenta Graffiana iteravit; neque VALISNERUS tot et tam variis in bestiis; neque ego in pene centum experimentis; neque nuperiorum anatomicorum quispiam, vesiculam, quales sunt in ovariis, post conceptionem, aut in tuba vidimus aut in utero. » Quoted by CRUIKSHANK (*op. cit.*, p. 199). CRUIKSHANK had an immense admiration for HALLER — « cujus auctoritas (he said, repeating another's saying) apud me plus valet quam auctoritas omnium aliorum anatomicorum simul sumptorum ». Happily this did not blind him to the facts.

all the fame it deserves. He described the germinal vesicle in the chicken's egg (*Symbolae ad ovi avium historiam ante incubationem*, Breslau, 1825; quoted in the facsimile below, p. 25), and thus by building so-to-say a bridge between the avian egg and the mammalian one, made the concept of the latter more plausible. Indeed mammalian ova as compared with bird's eggs were exceedingly small, of microscopic size. This is accounted by the fact that the bird's egg being expelled from the bird's body, must be entirely self sufficient and contain all the food which will be necessary to build up the embryo. But if one considers only the germinal part of it—and PURKINJE's discovery made this for the first time possible—the eggs of birds and mammals are brought much nearer to each other.

3. **Baer's own discovery.** — It is thus clear that v. BAER's great discovery did not come out of the blue sky,—no discovery ever does. The young Königsberg anatomist was well acquainted with the works of his predecessors. None of these had actually clinched the argument, and the best proof of it is that they had failed to convince anybody; they had not even completed their own conviction. Yet, thanks to their efforts the concept of mammalian egg had become plausible; it had been rescued from the oblivion to which HALLER's absolutism had condemned it. Prejudices were not yet overcome, but the question was re-opened and it was now possible for an intelligent and persistent man of science to investigate it anew, more thoroughly, and perhaps to solve it. This was v. BAER's achievement. His own account of his discovery is so moving that we cannot do better than to reproduce it verbatim (*Selbstbiographie*, 1866, 310-12):

«So standen die Untersuchungen über diesen Gegenstand, als ich mich in ihn zu vertiefen anfing. Im Jahre 1826 hatte ich schon mehrmals kleine durchsichtige Eier von 1/2 bis 1 1/2 Linien (5) Durchmesser, wie PRÉVOST und DUMAS sie gesehen hatten, in den Hörnern des Uterus und selbst in den Eileitern gefunden, im Frühlinge 1827 aber bedeutend kleinere, viel weniger durchsichtige und deshalb kenntliche in den Eileitern. Ich zweifelte nicht, diese auch für Eier zu halten, da es ja wahrscheinlich war, dass die Dottermasse auch bei Säugethieren ursprünglich undurchsichtig seyn werde. Ich sprach im April oder in den ersten Tagen des Mai des zuletzt genannten Jahres mit BURDACH darüber, dass ich gar nicht mehr in Zweifel seyn könne, die Eier der Säugethiere kämen fertig

(5) I. e., 1 to 3 mm. (G. S.)

gebildet aus dem Eierstocke, und dass ich sehr wünschte, eine Hündin zu erhalten, die erst vor ein Paar Tagen sich belaufen habe. Nach PRÉVOST's und DUMAS's Beobachtungen musste man nämlich glauben, dass man um diese Zeit bei Hunden die Graafischen Bläschen noch geschlossen finden werde, aber reif zur Eröffnung. Man glaubte damals, dass die Eröffnung der Eikapsel oder der Graafischen Bläschen unmittelbar von der Paarung abhänge, was nicht richtig ist. Zufällig besass BURDACH im eigenen Hause eine solche Hündin, die längere Zeit schon Hausgenossin gewesen war. Sie wurde geopfert. Als ich sie öffnete, fand ich einige Graafische Bläschen geborsten, keine dem Bersten sehr nahe. Indem ich, niedergeschlagen, dass die Hoffnung wieder nicht erfüllt sey, den Eierstock betrachtete, bemerkte ich ein gelbes Fleckchen in einem Bläschen, sodann auch in mehreren andern, ja in den meisten, und immer nur ein Fleckchen. Sonderbar ! dachte ich, was muss das seyn ? Ich öffnete ein Bläschen und hob vorsichtig das Fleckchen mit dem Messer in ein mit Wasser gefülltes Uhrglas, das ich unter das Mikroskop brachte. Als ich in dieses einen Blick geworfen hatte, fuhr ich, wie vom Blitze getroffen, zurück, denn ich sah deutlich eine sehr kleine, scharf ausgebildete gelbe Dotterkugel. Ich musste mich erholen, ehe ich den Muth hatte, wieder hinzusehen, da ich besorgte, ein Phantom habe mich betrogen. Es scheint sonderbar, dass ein Anblick, den man erwartet und ersehnt hat, erschrecken kann, wenn er da ist. Allerdings war aber doch etwas unerwartetes dabei. Ich hatte mir nicht gedacht, dass der Inhalt des Eies der Säugethiere dem Dotter der Vögel so ähnlich sehen würde. Da ich aber nur ein einfaches Mikroskop mit dreifacher Linse hingestellt hatte, war die Vergrösserung nur mässig und die gelbe Farbe blieb kenntlich, die bei stärkerer Vergrösserung und Beleuchtung von unten, schwarz erscheint. Was mich erschreckte, war also dass ich ein scharf umschriebenes, von einer starken Haut umschlossenes, regelmässiges Kügelchen vor mir sah, von dem Vogeldotter nur durch die derbe, etwas abstehende äussere Haut unterschieden. Auch die kleinen undurchsichtigen Eichen, die ich im Eileiter gefunden hatte, hatten nur eine gelblich-weisse Färbung gehabt, ohne Zweifel weil der Dotter schon in Auflösung begriffen war; die grössern waren durchsichtig. Es wurden noch mehrere solcher unaufgelöster Dotterkugeln ausgehoben, und alle auch von BURDACH gesehen, der bald hinzugekommen war. »

(The rest of v. BAER's account is equally interesting, but more technical.)

To those who would be tempted to give too much credit to CRUIKSHANK, PRÉVOST, and DUMAS, for their remarkable anticipations of this discovery, I would point out in this quotation, the sentences expressing v. BAER's emotion when he had just made it. When he first saw the minuscule sphere of yolk which was the egg he had been dreaming of, he was so struck that he was obliged to rest himself before he had the courage to look a second time into his microscope; he was afraid of having been deluded by a phantom. This proves satisfactorily that the discovery could not be considered as already completed at the time that v. BAER made it, for he who had read anterior observations, and had

pondered over them, was so far from being convinced by them that he was still in the expectant and adventurous stage of mind which his description reveals.

4. **Publication of the Epistola** (1827). — The discovery was made in April or in the early days of May. The author hastily completed it by a number of correlative observations, and wrote a long memoir on the subject. The Imperial Academy of St. Petersbrug had just elected him a corresponding member, and to return the compliment the memoir was written in the form of a Latin letter addressed to that Academy « on the genesis of the egg of mammals and of man ». This awkward title (as if man was not one of the mammals) was used by the author to assert that the discovery applied to man as well as to other mammals; though his observations were made primarily on dogs, he had actually observed the eggs of other mammals including man (see fig. XIII). The memoir was sent about the middle of July 1827 to the publisher LEOPOLD VOSS in Leipzig. For the amateur of bibliographical details, I may add that though its cover bears the date 1827, it did not appear until January 1828.

5. **Analysis of the Epistola.** — As this memorable publication is written in Latin, a brief analysis of it will increase the usefulness of the facsimile given below. The introductory letter addressed to the Russian Academy contains a brief history of the subject; note (on p. III) the discussion of the relation of the ova to the Graafian vesicles to which I shall come back presently. The treatise itself begins with a description of dog's embryos. In the second chapter, the earlier stages of the development of the dog's ova are considered. This may seem an unnatural order, but it was justified because it was better first to consider the more familiar objects, and then to pass on to the less familiar and the unknown. It would be too long to give an account of these observations, but they are not very difficult to follow with the help of the author's drawings, and his references to other observations can easily be detected even by one having but little knowledge of Latin. Of course one weakness of this memoir (and of many others for a long time afterwards) was the very imperfect dating of some of the embryonic materials, but this was unavoidable. Remember this was the very beginning

of modern embryology, and to make possible the dating—even approximate—of eggs and embryos of various animals considerable embryological experience was needed (unless they were dated by the very conditions of the experiment). In chapter 3, v. BAER studies the ova found in dog's ovaries; in chapter 4, the structure of the Graafian vesicles and the ova of mammals in general. This completes the analytical part of the work, which is followed by the synthesis divided into two chapters: 5. Sketch of the embryological development of mammals; 6. Comparison of the eggs of mammals with those of other animals. The writing of this last chapter was made possible by PURKINJE's discovery of the germinative spot in the chicken's egg; that discovery had been published in 1825 but v. BAER only heard of it in the autumn of 1826, a few months before his own discovery, when his mind was maturing for it.

His conclusions with regard to the relation of eggs to Graafian vesicles deserve to be translated verbatim (See p. 32; the underlining is the author's own).

« *When we take the ovary and in general the maternal organism into consideration, the Graafian vesicle is thus the real egg of mammals.* However, as far as its development is concerned, it diverges widely from the egg of other animals; these are carried intact out of the ovary, and they not only provide for the fetus a place of development, they transform themselves into that fetus. In the mammals the *embedded vesicle* contains a more developed yolk and *behaves with regard to the coming embryo as the real egg. It might be called the fetal egg within the maternal egg.* The mammals have thus an egg within an egg, or, if this way of putting it may be allowed, an egg in the second power. »

These considerations must seem very strange to present day embryologists, but to appreciate them correctly they must bear in mind many facts. To begin with, v. BAER passed the formative years of his youth—the years during which he finally found himself—in Würzburg and obtained there the best part of his scientific education under the guidance of the anatomist IGNAZ DÖLLINGER (1770-1841), and through DÖLLINGER he also imbibed SCHELLING's « Naturphilosophie ». It is not here the place to discuss the scientific consequences of that romantic philosophy; some were disastrous, but others were excellent, and among these must

be quoted the synthetic endeavors of such men as OERSTED and v. BAER.

In the second place, and perhaps more to the point, the understanding of the Graafian vesicles and of their relations to the embryos was vague and uncertain in v. BAER's time (see his preface, p. III). We may recall that a careful description of these vesicles was first made by the Dutch anatomist REGNER DE GRAAF (*De mulierum organis generationi inservientibus*, Leiden, 1672), and they were mistaken by him for eggs (6); hence they were at first called ovula Graafiana. HALLER and others investigated them and never observed these so-called eggs except in the ovaries; in particular they never found them either in the oviducts or in the womb; on the other hand, after a longer time, eggs or embryos were found in the womb. HALLER concluded, first, that these ovula were not eggs, and their name was changed into vesiculae (or folliculae) Graafianae, second, that the real eggs crystallized in the womb out of a fluid substance. The first conclusion was right, the second, wrong. Now the link between vesicles and embryo which HALLER had been unable to find, had finally been detected by v. BAER. This was indeed the essence of his discovery : he had seen the real eggs appear within the walls of the Graafian vesicles; he had followed them along the oviduct into the womb, where he had watched their further development. Thus HALLER was wrong : the eggs were not created in the womb, but in the vesicles. In his reaction against the great master HALLER, v. BAER was carried too far and returned with his enriched knowledge to DE GRAAF's original position, if not to a cruder one : the vesicles found in mammalian ovaries are equivalent to the birds' eggs. But then what of the new eggs discovered by him ? v. BAER saw no way out but to call them « eggs within eggs », « eggs in the second power », a feeble compromise to be sure. We must add that a deeper investigation of the facts was materially impossible without the histological knowledge and technique which did not become available until much later.

(6) DE GRAAF's interpretation was far more complex and nearer the truth than can be gathered from v. BAER's summary of it. According to DE GRAAF the vesicles were less eggs, than egg nests, but of course he had no idea of the nature of the egg itself nor of its ulterior development. See B. OTTOW's preface to v. BAER's memoir (Leipzig, 1927, p. VI).

I have discussed this question because of its psychological interest. A man makes a great discovery and misinterprets it because he is hypnotized by earlier ideas. The history of science is full of similar examples. It shows that the most difficult thing of all is to see things as they are, without preconception. Few people are able to do it at all, and these few only by intermittence.

I had another good reason for discussing it. v. BAER's hesitant explanation of his discovery is practically unknown. Indeed nobody reads his memoir of 1827, and the few embryologists and historians who have some knowledge of its contents, have obtained it from v. BEAR's autobiography. Now that autobiography (like all other works of the same kind) does not tell the whole truth. Though some twenty pages are devoted to the *Epistola*, no mention is made of the author's half-hearted interpretation of his own observations.

For other considerations on the Graafian and Purkinjean vesicles, on the corpus luteum and the thecae folliculi, I must refer to the original memoir (p. 25-35), as it is obviously impossible to discuss every point of it. I shall only quote the final conclusions :

« Every animal, which is begotten by a sexual union, develops out of an egg; none out of a formative liquor. (7)

« The male semen acts through the eggskin (cuticula ovi), which contains no aperture, upon the egg and especially upon a definite part of it.

« Every development strives from the center toward the periphery. The central parts are thus formed before the external ones.

« The process of development is the same for all vertebrates. It begins with the backbone. »

6. **Reception of v. Baer's discovery.** — In spite of the pains he had taken to re-explain his discovery in German in HEUSINGER's *Zeitschrift für organische Physik* (vol. 2, January, 1828), it made

(7) Let me repeat that the concept of formative liquor, whence the egg developed in the womb long after mating, was the accepted one in 1827. A few years before, in 1824, a memoir of HAUSMANN, explaining that concept had been crowned by the scientific society of Göttingen. This HAUSMANN, who became director of the veterinary school of Hanover, published an enlarged and improved edition of his memoir in 1840 ! (G. S.)

but little stir. Throughout the year 1828, the reaction toward his memoir was—as v. BAER's puts it—« altum silentium ». When he attended the meeting of the Versammlung der Naturforscher, in Berlin, in September of that year, none of the anatomists whom he met knew a word of it. He was too proud to mention the subject himself, and it was only on the last day that he was given a chance to demonstrate his views. This was due to the interest of the Swedish anatomist, ANDERS RETZIUS (1796-1860), whose mind was uncommonly versatile and receptive. Among the small group of scientists who attended his demonstration were JOHANN MÜLLER, ERNST WEBER, and PURKINJE.

The silence was broken in the following year (1829). One may say that v. BAER's discovery was on the whole well received, without special enthusiasm it is true, but with few discordant notes. A deeper appreciation of it happened only later, gradually, as embryological knowledge developed.

After the establishment of the cellular theory by THEODOR SCHWANN, in 1839, v. BAER's discovery and its abundant fruits were re-explained in the new language. (8) Of course this was not a new discovery, but simply a re-interpretation. It is interesting to note that embryology was already well advanced when the cellular theory was finally understood. This is an excellent illustration of the fact that discoveries do not necessarily follow one another in a logical order; the human spirit groping in the darkness does not follow by any means the shortest path.

7. **v. Baer's later work.** — I have devoted so much space to the *Epistola* of 1827 because it was truly the foundation stone of modern embryology. It must be judged in that light. Leaving aside chronological considerations, its intrinsic importance was much smaller than that of v. BAER's ulterior publications on the same subject. His main embryological work was the great treatise entitled *Über Entwickelungsgeschichte der Thiere. Beoachtung und Reflexion* of which the first part appeared in 1828 and the second in 1837 (both in Königsberg; for an account of them see *Selbstbiographie*, 1866, 297-300, 331-43, 389-99, 447-51). If the *Epistola* may be called the foundation stone of modern embryology,

(8) In the *Epistola* reproduced below note that the term oviculum is practically equivalent to our egg cell, while ovum refers to a later stage.

these two volumes were really the whole foundations of it. To be sure they embodied the investigations not only of the author but of many other scientists; they realized a great structural effort : the creation of a new branch of science. They contained a very elaborate account of one of the fundamental doctrines of embryology, the theory of germ layers (Keimblätter), which had been vaguely outlined by WOLFF and clearly explained by v. BAER's colleague, PANDER, in 1817. But they did *not* contain, except by way of general indication, the recapitulation theory —i.e., the theory that the embryonic development of each creature is a brief recapitulation of its ancestral history. That theory was elaborated by FRITZ MÜLLER (1821-97) in his book *Für Darwin* (Leipzig, 1864), and popularized by HAECKEL. (9)

As I have spoken above (§ 1) of the old conflict between the epigenesists and the « evolutionists », it is well to state v. BAER's attitude with regard to it. It was to some extent a compromising attitude. The egg and sperm do not inherit and contain organs, but in some way or another they must contain patterns, inasmuch as the development of a given kind of fertilized egg always occurs in the same way. To quote his own words (*Selbstbiographie*, 1866, 319) :

« Vorhergebildet (präformirt) ist das körperliche gar nicht, wohl aber ist der Gang der Entwickelung ganz der Gang, welchen die Aeltern durchlaufen haben.

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(9) The law of development formulated by v. BAER according to which the growing organism passes from the general to the particular, would be interpreted to-day in the light of the doctrine of recapitulation, but v. BAER did not. On the contrary he denied that doctrine for one reason because no adult animal ever has a yolk-stalk or is surrounded by amniotic fluid.

Note that denial of the doctrine implied that it was already in existence, and I have traced it back to JOHANN FRIEDRICH MECKEL (1781-1833) who wrote in 1811 (I quote from H. F. OSBORN : *From the Greeks to Darwin*, 2nd ed., New York, 1929, 308). « There is no good physiologist who has not been struck, incidentally, by the observation that the original form of all organisms is one and the same, and that out of this one form, all, the lowest as well as the highest, are developed in such a manner that the latter pass through the permanent forms of the former as transitory stages. »

This was generalizing on rather scanty foundations but the idea was there. Perhaps it even antedates MECKEL. I found a reference to a comparison made in 1793 by KARL FRIEDRICH VON KIELMEYER (1765-1844)—a very inspiring German biologist and philosopher who was CUVIER's teacher—of a tadpole to an adult fish. I wonder if that had much influence in the development of the doctrine.

(From a private letter by J. P. M.)

So ist grade das Unsichtbare, der Verlauf der Entwickelung vorausbestimmt, und giebt mit sehr geringen Variationen dasselbe Resultat, das der Entwickelungsgang der Aeltern gehabt hatte. Man kann also sagen, dass der Lebensprocess ein continuirlicher ist durch den ganzen Stamm der Nachkommenschaft, der nur von Zeit zu Zeit schlummert (in der Eibildung nämlich) und neue Individuen schafft, indem er nach einer Befruchtung seinen Verlauf neu beginnt, bei der Sprossenbildung aber seinen Verlauf nur fortsetzt, so dass die Sprossen, wenn sie vom Mutterkörper sich lösen, um ein eigenes Leben zu führen, den Anfang desselben nicht selbständig besassen, sondern ihn von dem Stammkörper hatten. Je niederer die Organisation eines lebenden Individuums ist, desto mehr ist die Fortpflanzung nur eine Verlängerung des individuellen Lebens, so dass auf den tiefsten Stufen des Lebens das Wachsthum ganz unmittelbar in die Theilung in mehre Individuen übergeht. »

8. **v. Baer's colleagues. The Baltic founders of modern embryology.** — If only to complete my account of the genesis of v. BAER's discovery, we must say a few words of at least two colleagues of his, who share with him the glory of being the outstanding embryological pioneers.

HEINRICH CHRISTIAN PANDER, born at Riga in 1794; studied in Dorpat, Berlin, and Göttingen, and finally, at v. BAER's suggestion, in Würzburg under DÖLLINGER. His investigation of the germ layers in the chick's embryo, published in 1817, was a product of his Würzburg days. In 1826, he was appointed a member of the Russian Academy, but resigned the following year. He died in 1865.

MARTIN HEINRICH RATHKE was born in Danzig, 1793; he studied in Göttingen under BLUMENBACH, then taught physiology in Dorpat, and after v. BAER's departure for St. Petersburg replaced him in Königsberg. He discovered the branchial slits in the embryo and the circulation ad hoc. This was a very startling discovery, which would become a splendid illustration of the future recapitulation theory. He continued his work in Königsberg until his death in 1860.

It is very remarkable that these three men, v. BAER, RATHKE, and PANDER hailed from the southern Baltic region. v. BAER was an Esthonian, PANDER, a Latvian, and RATHKE came from Danzig. The three were at a time connected with Dorpat; v. BAER and RATHKE taught in Königsberg; PANDER and v. BAER were members of the Russian Academy, which was thus continuing a fine tradition, for WOLFF had spent the last thirty years of his life in its service (1764-94). PURKINJE, who has been many

times mentioned above, and was also one of the founders, was a Czech (born in Leitmeritz, 1787; died in Prague, 1869).

9. **v. Baer's life.**—KARL ERNST VON BAER was born at Piep, in Esthonia, in 1792. His father was a small landowner. KARL ERNST was educated in Reval and Dorpat, and finally in Würzburg, where DÖLLINGER exerted a deep influence upon him, at once from the scientific and from the philosophic point of view. While in Würzburg, he collaborated with PANDER. After a brief stay in Berlin, he was invited by BURDACH in 1817, to join him in Königsberg. He remained in Königsberg seventeen years, being in turn prosector and professor. In 1834, he was appointed librarian of the Russian Academy. From then on the greatest part of his life was spent in St. Petersburg, until his retirement in Dorpat where he died in 1876.

The golden years of his life were undoubtedly the seventeen years spent in Königsberg. His main work was done then and there, and his fame is based upon his activities of that time, 1817-34. After the publication of the second part of his *Entwickelungsgeschichte*, in 1837 (which was really a fruit of the Königsberg period), his interest was gradually withdrawn from embryology, and devoted to zoology (chiefly ichthyology), geography, anthropology, and to historical or philosophical questions. It should be noted that his anthropological curiosity, though greatly fostered by the exploration of the immense Russian territories in which he took part, was not a novelty. In fact in 1824 he had already published an elaborate treatise on the subject: *Vorlesungen über Anthropologie für den Selbstunterricht bearbeitet*. Erster Theil (XXVI + 525 p., 11 pl., Königsberg, 1824).

During the Petersburg years, v. BAER traveled considerably and yet found time to write a large number of papers and books on many subjects, in German and in Russian. Though his activity was then of great importance for the organization of Russian science, no publication of that time does begin to compare with those which were written or matured in Königsberg.

It is typical of v. BAER's philosophical orientation that in his old age he wrote a couple of papers against Darwinism (reprinted in his *Reden*, vol. 2, 1876, p. 170-480) in the name of conformity to natural law and « Zielstrebigkeit ». In this however his fortune

was very similar to CUVIER's. Even as some of the best arguments for the theory of evolution were found in the latter's own contributions, v. BAER became, in spite of his anti-Darwinian theories, one of the apostles of the new biological philosophy. Indeed one of the best approaches to that philosophy is the embryological and v. BAER was naturally considered the fountain-head of that particular line of argument. This is legitimate, even if one does not ascribe to him the famous recapitulation theory which became the favorite war horse of the evolutionists. HERBERT SPENCER derived his theory of evolution from the homogeneous to the heterogeneous in part from v. BAER's investigations.

This is an additional proof of the preeminence of observations over theories. The anti-Darwinian v. BAER was not simply the founder of modern embryology, he was one of the pioneers of the evolutionary philosophy.

10. **Bibliography.** — The fundamental source is the Selbstbiographie published in 1866 « *Nachrichten über Leben und Schriften des Herrn Geheimraths Dr. Karl Ernst von Baer, mitgetheilt von ihm selbst.* Veröffentlicht bei Gelegenheit seines fünfzigjährigen Doctor-Jubiläums am 29. August 1864 von der Ritterschaft Ehstlands » (XVI + 519 p., St. Petersburg, 1866). With portrait and catalogue raisonné of his writings. A second edition, not available to me, appeared in Brunswick, 1886.

v. BAER's main philosophical and historical publications were collected in his *Reden gehalten in wissenschaftlichen Versammlungen und kleinere Aufsätze vermischten Inhalts* (3 vols., St. Petersburg; vol. 1, 1864; vol. 2, 1876; vol. 3, 1873). The following papers deal with historical subjects. In vol. 1, on SWAMMERDAM, SAMUEL THOMAS von SÖMMERING, ALEXANDER VON HUMBOLDT. In vol. 3, on the views of the ancients concerning swan song, the travels of ODYSSEUS, the commercial road crossing Russia in the fifth century B. C., SOLOMON's Ophir. Vol. 2 contains only philosophical papers, e.g., his anti-Darwinian contributions.

Fragments relating to philosophical zoology, selected from his works, in the *Scientific Memoirs* edited by ARTHUR HENFREY and THOMAS HENRY HUXLEY (p. 176-238, London, 1853).

The Latin memoir reproduced below was republished in German form, somewhat expanded, by the author himself in the *Zeitschrift*

für organische Physik (vol. 2, 125-193, 1828). This was a short lived periodical edited by C. FRIEDRICH HEUSINGER (3 vols., 1827-28). It was translated into French by GILBERT BRESCHET *Lettre sur la formation de l'œuf dans l'espèce humaine et dans les mammifères* (84 p., Paris, 1829), and into German by B. OTTOW : *Über die Bildung des Eies der Säugetiere und des Menschen*. This German version was published in 1927 by the publishers of the original memoir (LEOPOLD VOSS in Leipzig) as a part of the centennial commemoration (*Isis*, 12, 381).

REMIGIUS STÖLZE : KARL ERNST VON BAER *und seine Weltanschauung* (XI + 688 p., Regensburg, 1897). WILHELM HAACKE : K. E. v. BAER (VII + 175 p., *Klassiker der Naturwissenschaften*, 3, Leipzig; preface dated 1905).

Studies on v. BAER by V. VERNADSKIJ, M. SOLOVJEV, and E. RADLOV, were published by the Russian Academy (71 p. in Russian, Leningrad, 1927).

11. **Iconography.** — The portrait of v. BAER which we reproduce bears the inscription « Stich und Druck v. WEGER in Leipzig »; it was published in the first vol. of *Reden* (St Petersburg, 1864); also in the *Selbstbiographie* (St Petersburg, 1866). Of the monument which was erected in Dorpat to celebrate his memory, I have a faded photograph made by TH. JOHN, Alexander Strasse, Dorpat, in or after 1876. Our reproduction was made from a new photograph kindly sent to us by the librarian of the University of Dorpat (Tartu).

12. **Thanks.** — The present facsimile— no. XIII of our series of scientific classics—was made upon the original copy belonging to the Harvard Museum of Comparative Anatomy whose courtesy is hereby acknowledged. It was photostatted at the Library of Congress, thanks to Mr. F. E. BRASCH's kindness. The facsimile is complete but somewhat reduced, the original being of quarto size (30 × 23.5 cm.). Professor J. PLAYFAIR MCMURRICH, of Toronto, kindly read my manuscript, and added a note on the theory of recapitulation (see p. 326).

November 9, 1929. GEORGE SARTON.

DE

OVI

MAMMALIUM ET HOMINIS GENESI

EPISTOLAM

AD

ACADEMIAM IMPERIALEM SCIENTIARUM
PETROPOLITANAM

DEDIT

CAROLUS ERNESTUS a BAER

ZOOLOGIAE PROF PUBL. ORD. REGIOMONTANUS.

CUM TABULA AENEA.

LIPSIAE, SUMPTIBUS LEOPOLDI VOSSII.
MDCCCXXVII.

CAROLUS ERNESTUS A BAER

DE

OVI

MAMMALIUM ET HOMINIS GENESI.

VIRI EXCELLENTISSIMI PERILLUSTRES!

Suffragium VESTRUM, quo me *Academiae* ab epistolis esse voluistis, quantum me extulerit, VOBIS enunciare haud conabor, inprimis cum me, hominem novum, cum viris conjunxistis, quorum nomina plurimi ponderis sunt in republica literarum. Vos me adhortari voluisse puto, ne otier, sed intentis viribus tantorum virorum vestigia premam. Gratias pro VESTRA erga me benevolentia non aptius me acturum censeo, quam si VOBISCUM inventum novissimum communico, quod, cum ad historiam evolutionis pertineat, sub VESTRIS auspiciis in lucem proferre summam mihi praebet voluptatem. VESTRA enim *Academia*, ubi de investigandis naturae mysteriis in fabricandis novis corporibus organicis sermo fit, prae ceteris quam multum emineat, quis est, qui nesciat! Observatores, qui historiae evolutionis animalium prima fundamenta solida posuerunt, VESTRAE *Academiae* membra vel fuerunt vel adhuc sunt: vir sempiternae gloriae CASPAR FRIDERICUS WOLFF, cui ingenio paucos, perseverantia vero in investigandis rebus subtilissimis nullum parem vidit orbis terrarum, cujus nomen sine religiosa illa verecundia enunciare nequeo, qua res divinae originis veneramur; et CHRISTIANUS PANDER, ad cujus clarissimas et lucem tenebris afferentes disquisitiones de pulli evolutione ansam qualemcunque beato illo tempore praebuisse, quo IGNATII DOELLINGER, magistri nostri ad cineres usque, nunquam vero pro meritis satis colendi, institutione et consuetudine usi sumus, semper gloriabor.

Contigit mihi etenim, ut ovi mammalium et hominis primordia in ovario invenirem, quae per saeculorum seriem tot et tantis laboribus frustra quaesita sunt. Circa hanc rem naturae scrutatores quam maxime dissentire inter omnes constat.

Cum jam ex antiquitate notum esset, vitellum avium in ovario maturescere, tum per oviductum devectum, albumine et in illius parte inferiori vel sic dicto utero testa indui, Regner de Graaf, vesicularum in ovario mammalium si non inventor, primus tamen accuratior observator, non potuit non has pro ovis habere; et cum praeterea ova in cuniculorum tubis vidisset, nullus dubitavit, haec ova easdem esse vesiculas delapsas. Cui sententiae, a multis physiologis receptae, posthac contraria exstiterunt exacta experimenta Guilielmi Cruikshank, qui utique saepius ova in tubis cuniculorum, vesiculis Graafianis tamen multo minora, observavit. Nostris temporibus Cruikshankiana asserta a duobus observatoribus accuratis et microscopii usui quam maxime assuetis, Prévost et Dumas, comprobata sunt in canibus et cuniculis. Nescio ergo, quo fato seductus sit auctor novissimus, qui antiquissimam Graafianam sententiam pro nova vendidit, observationum nulla habita ratione.

Complures anatomici saeculo decimo septimo ova mammalium frustra in tubis quaesiverunt, et tandem praeeunte acerrimo Philippo Jacobo Hartmann Regiomontano, fidem omnem observationibus Regneri de Graaf denegarunt. Pauciores et jam cautiores saeculo decimo octavo et nono eodem modo contra Cruikshank egerunt.

Sic lis adhuc sub judice est. Id tantum extra omnem dubitationis aleam positum videtur, vesiculam Graafianam aliquid ad ovi evolutionem conferre, quia post conceptionem in corpus luteum mutatur. De modo, quo ex vesiculis ova oriantur, duae nostris temporibus valent opiniones potiores inter anatomicos. Alii putant, vesiculas Graafianas vitellis avium penitus respondere et fluidum innatum membranula circumdatum, ergo sub forma ovi, a tuba recipi. Hanc sententiam ipse hucusque fovi ob conspicuam similitudinem inter ovaria mammalium et avium fetumque evolutionem. Alii humorem tantum vesicularum ejici et per se vel cum semine virili mixtum ovum formare inter tubas credunt; ne eos dicam, qui spretis observationibus laudatis, et nulla ratione fetum extra-uterinorum habita, ova in utero gigni putant. De ovulis a vesiculis diversis in ovario obviis vix mentio fit ab auctoribus, inter quos unum tantum inveni, qui ovula vera fortasse vidit, sed tam male descripsit, ut observata ejus omnino neglecta sint.

Quibus praemissis ea, quae ipse cognovi, enarrare liceat. Incipiamus ab ovis maturioribus et ab his ad juniora transeamus, quia hac via ea, quae demonstrabo, clariora fient. Eandem viam in disquisitionibus ipsis sequi conatus sum, quantum potui. In his vero tantum ab occasione pendere, ut non omnia una serie persequi possimus, neminem fugit.

Canis ova potissimum examinavi. Ovorum et fetuum plurimorum aetatem indicare non potui. Ubi de hac re certior eram, cum observationibus a Prévost et Dumas institu-

tis eam non consentire expertus sum. Quod num de coeli diversitate pendeat nec ne, nescio. Relationem hac ex causa non ad tempora sed ad evolutionis statum dedi.

Geneseos ovi mammalium historiam me ad gradum quendam eruisse spero, et quae de ea docui, errore graviori premi non timeo. Quae vero ultima paragrapho de ovis animalium inferiorum ordinum protuli, eum in finem tantum dicta esse accipiatis, ut concordiam vesiculae Graafianae cum ovo avium et reliquorum animalium demonstrarem, iterum iterumque Vos rogo. Quam concordiam observationibus prosequens tanta in ovis reliquorum animalium inveni, quae hucusque omnino ignota erant, ut naturae scrutatores ad hujus generis investigationes adhortari haud ineptum mihi videretur. Me hanc disquisitionem non absolvisse, sed inchoasse tantum, licet non parvum ovorum numerum in diverso evolutionis statu observaverim, haud ignoro. Observata supprimere tamen nolui, quia ad comparationem propositam sufficiunt et quia optime didici, nec unius viri nec unius anni esse, hanc disquisitionem spinosissimam et immensis sane difficultatibus pressam absolvere. Ova enim immatura animalium inferiorum non tantum exilitate et tenuitate oculos et instrumenta nostra ludibrio faciunt, ut verbis viri in subtilibus disquisitionibus anatomicis versatissimi utar, sed et inter se valde discrepant. Maxima vero difficultas in eo nititur, quod praeter gallinas de nullo fere animali completam ovorum seriem nobis comparare possimus.

§. 1.

Fetus canini primordia.

Jam primo fere impetu, quo a collega illustri BURDACH monitus, ut ad cognoscendam mammalium evolutionem aliquid conferrem, his investigationibus me dare inceperam, canem praegnantem observare licuit, cujus ova multum profuerunt in comparanda evolutione mammalium cum eadem in avibus. Imo, anatomen hujus evolutionis gradus pene absolverim, si tum temporis jam satis in his disquisitionibus versatus fuissem. Ex quatuor ovis vero bina, mala methodo usus, destruxi, quo factum est, ut areae vasculosae ambitum indicare nunc nequeam. In uterum enim cani vivae ereptum, omni vitalitate praeditum, incidi, quod hodie valde dissuadeo. Jam in reliquis mammalibus difficile scopum hac via attinges, in cane vero semper ova juniora contractionibus uteri dilacerantur, quod et iis accidisse videtur, qui pluribus hebdomadibus post conceptionem ova in utero mammalium non invenerunt. (*Göttinger gelehrte Anzeigen vom Jahre* 1824, *pag.* 195). Tertium ovum post duas horas ex utero, vitae jam experte, enucleare studui. Uteri cornu supra ovum cautissime scisso, ovi membranam externam villis numerosis praeditam intactam distincte conspexi, quam vero, cultro omnino seposito contemplatus, uteri marginibus, qui villis illis arctissime jam juncti erant, retractis, disrumpi vidi! Fetum tamen integrum inveni et aream vasculosam splendide rubentem admiratus sum. Quartum denique ovum post meridiem ex utero jam paululum flaccido integerrimum evolvi. In examinandis et delineandis membranis aliisque rebus horas plures posui, quo factum est, ut, cum ad contemplationem areae vasculosae per microscopium me converterem, illius ambitum haud amplius discernere possem et nunc in dubio relinquere coactus sim. Sed methodice enarrabo observata!

Trium hebdomadum graviditatem dixit canis venditor, quod cum observationibus BOJANI (*in Nocis Actis nat. Cur. Tom. X.*) ex tempore

ulteriori, et conjunctorum amicorum PRÉVOST et DUMAS ex tempore antecedenti (*Annales des sciences naturelles. Tome III.*) satis bene congruit, minime vero cum reliquis meis experimentis, quae viginti quatuor circiter dies his ovis darent.

Mitto uteri conditionem et omnia, quae non pertinent ad propositum nostrum. Ovum magnitudine sua depictum videtis *Fig. VII.*, binis apicibus pellucidis, altero acutiore praeditum. Praeter apices totum ovum obscurius est villis membranae externae affixis, nam zona placentaria maximam ovi partem primitus cingit, uti jam ex BOJANI iconibus constat. Tunicae duae praeter apices satis distant et facile discernuntur. Quarum exterior, *chorion* vulgo dicta, omnino vasorum expers villos gerens, tunicae corticali vel membranae putaminis (*Schalenhaut*) avium respondet, quae non secius villos gerit, in testa calcaria reconditos. Villos chorii, eorum forma in homine et fugitiva examinatione seducti, anatomici nonnulli pro meris vasorum finibus vel pro vasorum fasciculis habent. Quam sententiam facile et in aliis mammalibus, verbi causa in suibus et ruminantibus, refutandam ovum nostrum optime dissolvit, nam villos hic ex tela formativa constitutos esse sine vasorum vestigio microscopium extra dubitationis aleam ponit (cf. *Fig. VII**). Posthac vero vasorum ramis fundum fertilem praebent. Hanc *membranam corticalem* in posterum nominabo, cum vox „*chorii*“ anceps sit, et si eam more solito adhibere placet, inde ab eo tempore adhibeatur, quo vasa insunt, ab allantoide, me judice, suppeditata.

Membrana interna utique saccum constituens, paululum flavescit et granulis superficie sua interna obsita est. His jam *membranam erythroidem* vel *vesiculam umbilicalem* se probat, quibus vasorum rete et ratio ad embryonem accedit. Ut nomine utamur, quod et iis animalibus adhiberi possit, quibus umbilicus verus deest, *saccus intestinalis* (*Darmsack*) apud nos audiat.

Embryo, quatuor lineas curvatura aequans, inter *tunicam corticalem* et *saccum intestinalem* locum tenet, cum sacco dicto ita concretus ac pullus gallinaceus exeunte die tertio. Eodem modo parte sua posteriore recta ($2\frac{1}{2}$ linearum) ventri aperto incumbit, dorsum jam clausum tunicae corticali advertit, parte anteriore, vagina capitis obtecta, dextrorsum et deor-

sum versa, lateri sinistro incumbit, ubique area vasculosa circumdatus. *Figura* nostra 7. fetum in situ naturali exhibet, sed pars antica justo brevior videtur, quia sacco intestinali cum ambiente capitis vagina immersa est.

*Figura VII*ª. fetum pingit cum parte sacci intestinalis ita sub microscopio positum, ut pars anterior penitus et pars posterior paululum lateri sinistro incumbat. In hac positione mirus consensus inter evolutionem pulli avium et mammalium optime perspicitur. — Incipiamus cum vagina capitis *b c d e*, cujus dimidium fetus caput et collum dextro latere obtegens resecui, ut partes hae clarius in lucem venirent, illaeso tamen venae descendentis dextrae trunco, quem reclinavi (*e f*) ne ejus connexus cum cordis atrio, trans vaginae initium, bene visus, turbetur. Locum, quem ante reclinationem tenuerat, signo *f* notavi. Denudata itaque corporis parte curvata optime in oculos cecidit cordis atrium (*e g*) venas recipiens, et ventriculus (*h*) a sinistro ad dextrum latus in spiram tortus et ita ac in pullis avium, deficiente pectore, a corporis lateribus non tectum. Statum hunc cor denudatum dicere solent; attamen vere nudum cor esse non potest, quando capitis vagina jam pectoris regionem attigit, quare cor, si unquam, per brevissimum certe temporis spatium, et tantum in parte venosa, vere nudum est. In nostro fetu praeter cuticulam pectus tegentem jam tenerrimum pericardium (*i i*) adest, quod ante atrium et ventriculum distinguitur. Ex corde systema arteriosum emergere vidi, quod utrimque in quatuor arcus finditur, tum, uti in piscibus, sub columna vertebrali in truncum communem (*l*) — Aortam — collectos. Ex arcu anteriori ramus ad capitis partem anteriorem — ad oculum fere — vertitur, secundus arcus ante auris vestigium, tertius pone idem organon ramulum mittit. Primus et secundus arcus utriusque lateris ex bulbo quodam (*k*) emergunt. Idem systema vasculosum in pullis gallinaceis saepius observaveram et jam aestate 1826, antequam Huschke inventa sua publici juris fecerat, exercitationes meas cum Rathke communicavi, adjiciens me putare, foramina ab eo detecta huic fabricae originem debere, de quo posthac penitus certiorem me feci (Isis 1825, p. 747 et 1100). Nunc haec scribendo idem systema vasorum in embryonibus tenerrimis Colubri Natricis et Lacertae agilis parum discrepans observavi. De ranis

et salamandris jam dudum innotuit. Omnibus ergo animalibus vertebratis in statu fetali talis convenit vasorum apparatus et in omnibus sub capitis parte posteriori situs est, uti et in nostra figura auris locus docet. Imo cordis ventriculus, ne dicam capiti, sane collo respondet. Arcus arteriosi initium oesophagi circumdant, cujus adítus, oris locum tenens, ante bulbum *k* apertus visus est. Spinae dorsi et medullae spinalis cum cerebro curvaturas nec non vertebrarum primordia ita me pinxisse puto, ut commentario non egeant. Auris jam distinctissima medullam oblongatam indicat, quae, ni fallor, minus a medulla spinali differt, quam in pullis avium ex eodem evolutionis statu.

Jam ad partem rectam transeamus. Intestinum totum, ultimo fine excepto, apertum est et ex asse intestinum avium, uti a C. FRIDERICO WOLFF ex die tertio absoluto subtilissime describitur, et pingitur (*Novi comment. academ. Petropolit. Tom. XII.*), refert. Semicanalis enim est, cujus fundus sulco levi notatur, quem WOLFF suturam nominat et ex coalitione duarum plicarum amnii sui spurii ortum putat, qui vero inde oritur, quod sub rhachide binae taeniae sacci intestinalis e situ horizontali in situm perpendicularem vertantur, parietes inde semicanalis constituentes dum majorem crassitiem induunt, et magis magisque angulo et tenuissimo toro transitum in saccum intestinalem notent. Sectione transversa haec optime demonstrantur. Sic *Fig. VII*ᵇ. intestini fundum (*suturam*) ad *f*, et angulum, de quo locuti sumus, ad *e* exhibet. In *Fig.* nostra *VII*ᵃ. *e p* fundus vel sutura intestini est; utrimque fascias vel taenias *s t*, *q r*, intestini nascentis parietes, acu paululum separavi. Fascia sinistra *e p q r* angulo *q r* in saccum intestinalem (*w v*) transit. Ab angulo (*s t*) fasciae dextrae saccum intestinalem usque ad *r* desecui, ne fetus latus cum amnio veletur; vasorum origines tamen indicare aptum esse putavi. Utrimque enim in sacco intestinali rete vasorum (*w v*) apparet, quod per trunculos plures (quorum septem ad latus sinistrum, sex ad dextrum numeravi) cum truncis majoribus in fetu sitis communicatur. Regio inter rete vasculosum et fetum (*q r w*) areae, quam pellucidam dicunt in avibus, respondet, licet hic pluribus vasis gaudeat. Rete vasculosum aream vasculosam avium sistens per plures horas optime quidem

vidi, fines tamen exteriores jam discernere non potui; minoris fuisse ambitus, ratione ad fetum habita, quam in avibus, jam inspectione tertii ovi ante microscopium adhibitum persuasum mihi habeo. Et sinum terminalem me vidisse memini, de quo eo minus dubito, cum et Saurii et Ophidii eo non careant. Intestini parietes *q r* et *s t* jam crassiores erant, quam ut vasorum truncos sub iis reconditos clarissime discernere et in toto nexu persequi potuerim, duos tamen vidi sanguinis haud expertes, quos pro aortae ramis principalibus habui (de quibus cf PANDER, *Beiträge zur Entwickelungsgeschichte des Hühnchens im Ei. — Würzburg* 1817. *Fol. Tab. octav.*); venae scilicet in hac periodo sanguinem citius amittere solent ac arteriae, ob cordis vim sane suctoriam diutius persistentem quam vita in ramulis minoribus. Trunculos ergo descriptos (*w*) pro arteriis sumsi, licet venae inter eos esse possint. Vas vero a posteriori fine circa caudae involucrum decurrens tum ab intestini crassi aditu (*r*) oculis subductum, vena visum est adscendens. De venis binis descendentibus jam locutus sum. In vagina capitis minores arteriarum fines adfuisse haud dubito.

Amnion dorso tam arcte incubuit, ut ante incisionem iis tantum locis discerni posset, quibus curvaturam quandam fetus habet, v. g. in medio dorso, si non fallor, ad transitum medullae oblongatae in corpora quadrigemina (**) et ad horum transitum in cerebrum (*), potissimum vero ad fetus finem posteriorem, ubi involucri caudae laminam format superiorem. Ubique jam clausum erat et suturam haud amplius inveni. Praeterea eadem ratio ejus ad fetum atque in avibus, de quo conf. sectionem transversam (*Fig. VII b.*). Nullus ergo dubito eodem modo evolutum esse.

Sacculus minimus (*z*) ex apice caudali vix emergens et involucrum caudae in suas laminas findens sine dubio membrana *allantoides* est, quam, cum erythroidem saccum intestinalem nominaverim, *saccum urinarium* (*Harnsack*) dici velim.

Partium internarum conformatio haud minus cum avicularia congruit, uti potissimum sectio transversa docet, quam in medio dorso factam *Fig. VII b.* pinxi. Dorsum ex concretis duabus laminis dorsalibus (*plicis primitivis* PANDERI) (*a b*) medullam spinalem (*k*) circumdantibus

constituitur, laminis ventralibus (*b c*) adhuc summopere distantibus. Sub canali pro medulla spinali columnae vertebralis prima vestigia apparent (*g*) e tela formativa densiore concreta, quae ramos breves sursum in laminas dorsales mittunt. In media columna vertebrali *chorda dorsalis* tenuis obscuritate sua in conspectum venit. Est enim in omnibus animalium vertebratorum fetibus (uti in mammalibus, avibus, ophidiis, sauriis, batrachiis nec non piscibus observavi) chorda quaedam teres a cauda ad caput usque, in quo cum tuberculo desinit, per mediam columnam dorsalem extensa, ab hac soliditate haud minus ac gracilitate toto coelo diversa et ante vertebrarum vestigia oriunda; cum columna cartilagineo-ligamentosa, quae in piscibus cartilagineis nonnullis, v. c. in Sturione et Petromyzonte, per totam vitam in spina dorsi obvia est, penitus congrua. In pullis gallinaceis jam primo incubationis die formatur longe ante laminarum dorsalium coalitum. Haec *chorda* hucusque penitus fere neglecta (Dutrochet solum eam cognovisse sed non in primordio, ex observationibus ejusdem circa ossium genesin in salamandris video) saepius fefellit naturae scrutatores. Illa sine dubio est, quam Pander pro medulla spinali habuit: nam medullam spinalem non ante laminarum dorsalium coalitum apparere pace clarissimi Serres certo certius compertum habeo, et ubi apparuit, acu nunquam piscanda est. Eandem chordam Prévost et Dumas in ranis etiam pro medulla spinali habuerunt. (*Annales des sciences naturelles. Tome II. p.* 113. *seqq.*)

In his omnibus gravioribus ergo embryo caninus cum aviculario congruit et differentia in mutua tantum partium ratione versatur. Sic transitus dorsi in nucham magis curvatus, cerebrum vero minus a medulla spinali differre videtur in illo quam in avibus eundem metamorphoseos gradum assecutis. Area pellucida et area vasculosa minores et rami arteriosi plures adsunt in cane. Quo modo hi in vasa omphalo-mesenterica vertantur, ulterioribus examinibus dijudicandum relinquo.

Quae cum ita a me essent inventa, valdopere optavi ova examinare, quae medium tenerent inter modo descripta et ea, quae Prévost et Dumas ut 12 dierum ova descripserunt et pinxerunt. Canem ergo dissecui per duas hebdomades et quod excurrit fetam. Sed ova in utero

libera parvula, diametro lineam dimidiam explentia, sine ullo fetus vestigio inveni, de quibus infra sermo fiet. Altera canis ejusdem graviditatis temporis eadem dedit ova. Canem denique cultro anatomico subjeci, quam optime scivi (in mea enim domo vivebat) ante viginti dies prima vice et ante septendecim dies ultima vice cum mare coivisse. Denuo spes me fefellit. Ova duarum linearum diametri jam tam arcte utero cingebantur, ut irrita omni cura dilacerata sint. Quae melius observare licuit, fetum nondum fovebant; in uno tantum stria tenuis (*chorda dorsalis?*) cum area quadam opaca fetus primordia indicare videbatur. Nec blastoderma clare apparuit et circa hanc partem omnino dubius haererem, nisi eam in cuniculis vidissem. Ex his ova ejusdem evolutionis facile et sine laesione evolvi, quia *membrana corticalis* crassior et minimis tantum villis obsita erat. In ovis nostris caninis *membrana corticalis* tenerrima villis suis magnis cum uteri cingulo tam arcte cohaerebat, ut vix ars ulla eam sejungere posset. Si ea mente recordor, quae de ovis 12 dierum Prévost et Dumas docent, suspicarer in mea cane ovorum tunicas praeter naturam et suppressa fetus evolutione crevisse; villi enim jam eos fere aequabant, quibus ova initio descripta obsita erant.

Praeterea iconès fetuum caninorum, quas laudati scrutatores Gallici exhibuerunt, cum meis observationibus ex ulteriore et antecedente evolutionis statu tam bene congruunt, ut non haesitem, unam in tabula nostra *Fig.* 6. et *VI.* repetere ad complendam seriem formarum. Docet enim fetum caninum eodem modo ac pullos avium oriri.

Quibus animo perpensis non dubitavi ovum mammalium in ovario eandem habere conditionem, quam ovum avium. Longe alia docuit observatio!

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§. 2.

Ovi canini evolutio primitiva.

Praeter ova modo descripta saepius alia vidi; penitus pellucida, multo minora, dimidiam lineam diametro aequantia (*Fig.* 5), quae tam libere

in uteri cavitate jacebant, ut levissimo flatu in hujus organi muco moveri vel cultelli manubrio facillime attolli possent, atque tam tenerrimis membranis constituta erant, ut quivis flatus formam mutaret eodem fere modo ac bullae saponariae, quibus fabricandis et per aërem agitandis pueri gaudent. Sub microscopio tale ovum haud penitus globosum inveni, sed paululum in longitudinem potius ductum. Primo momento simplex videbatur tunica, mox vero et jam ante eam temporis partem, quae minuta dicitur, lapsam interna quaedam lamina sejungi coepit ex oppositis finibus, uti *Fig. V*^{b}. docet. Vacuum inde utrinque ortum est sigmoideum. Ambo haec vacua tum pedetentim increverunt usque dum se tangerent, ac post semihoram jam tota membrana interna ab externa sejuncta reprehendebatur, una tantum regione excepta, qua cohaerere pergebant. Adspectus ovi ideo diversus erat dum acu sub microscopio vertebatur; mox enim aream opacam in medio sitam et tenui annulo pellucido cinctam ostendit, mox vero orbem opacum in orbe lucido ita situm ut hinc se tangerent, inde vacuo magno semilunari sejungerentur, prout regio adhaesionis in axi a microscopio per ovum ducta sita erat nec ne. Omnes hos adspectus pingere nec tabulae ratio sinit nec utile visum est. Potiores vero habetis, si *Figuras IV*^{a}. *IV*^{b}. *V*^{a}. *V*^{b}. inter se comparare placet. Idem enim fit in ovo minori, infra describendo. Iam ante horam primam membrana interna collabitur ita, ut pedetentim sphaericam formam omnino deponat speciemque sacci in quovis ovo alio modo collapsi induat. Haec dum fiunt, membrana externa initio formam servat sphaericam, tum etiam collabi incipit et post horas nonnullas rugis coarctatur irregularibus. Tale ovum *Fig. V*^{a}. pictum videtis, cujus diametrus a dimidia lineae parte ad tertiam ejusdem mensurae partem diminuta est.

Quaenam hujus phaenomeni causa sit, VESTRO judicio dijudicandum submitto. Calorem animalem aqua frigida, in qua ova sub microscopio contemplari solemus, deprimi indeque tensionem et ambitum ovorum diminui, infitias non ibo. Plurima scilicet ova ex uteris depromsi calore animali non penitus destitutis. Sin vero refrigeratio ad efficienda phaenomena relata sufficeret, cur, quaeso! ovum non statim deprimitur, cum tam parva vesicula, acus capitulum magnitudine non excedens, jam primo

temporis momento refrigeretur necesse sit? cur non totum ovum deprimitur uti praeparata anatomica cava, quae, si halitu humano extenduntur, mox collabuntur, vel si aestate consecta et caute aëre atmosphaerico expansa formam siccando bene servant, hieme tamen magnas impressiones nanciscuntur. Ut observationibus de hac re aliquid statuerem, semel trihorio post uteri excisionem ejus cornu aperui et ova flaccida quidem inveni, membranae vero, exceptis finibus oppositis, cohaerebant (ut in *Fig. V* [4].), et nescio si non sejunctio in finibus eo tempore sit facta, quo microscopium adaptavi. [*]).

Refrigerationem ergo ad membranarum separationem et collapsum haud sufficere arbitror, praesertim ea perpendens, quae de serpentibus lacertisque narrabo. (Cf. §. 6.). Membrana externa potius aquam imbibere dum vitalitatis aliquid inest, et saccus internus halitum qualemcunque vitalem amittere videtur, qui ab aqua fortasse extrahitur. Id praeprimis ova cuniculi duas lineas diametro aequantia, parvulis villis ex parte jam obsita docuerunt, cum in iis, collabenti membrana interna, externa non tantum turgida maneret sed magis tendi videretur.

Membranas accuratius perlustrando exteriorem pellucidam inveni et tuberculis minimis semipellucidis obsitam, primis forsan villorum vestigiis; nimis enim adhaerebant, quam quae pro corpusculis extraneis fortuito agglutinatis habeantur. Numerus eorum parvus et uti forma valde variabilis. *Membranam corticalem* esse nullus dubito. Bis sub microscopio multum amplificante duas discrevi striulas concentricas ac si ex duabus laminis conflata esset. Cum vero simplex membrana sphaerica, quae utrimque aream habet pellucidam, si sub microscopio valde amplificante observatur, duas necessarie exhibeat umbras, externa scilicet et interna facie, ex hac observatione circa membranae laminas nihil colligi posse censeo.

[*]) In alterum uteri cornu me post 30 horas mense Junio, tempestate calidissima, incidisse, sed ne unum quidem ovum vidisse, licet quatuor corpora lutea in eodem latere affuerint, eorum in usum, qui talibus observationibus operam dare in animo habent, refero, ne incisionem justo longius differant. Ova tum tenuiora cito corrumpuntur.

Membrana interna sub lente simplici lepidissimum praebet adspectum dum turget. Annulis enim vel sphaerulis, centro pellucido peripheria obscuriori praedita videtur. Quae maculae, si majori microscopii vi subjiciuntur, aliam exhibent formam ac si metamorphosin passae essent. Sunt enim nec sphaerulae nec veri annuli, sed ex grânulis potius constituuntur in orbes irregulares dispositis (*Fig. V.*), unde sub minori visus angulo (*Fig. V.*) orbes continuos esse mentiuntur. Oculus microscopicis observationibus adsuetus facile cognoscit, has maculas non ipsi membranae esse innatas, sed ejus superficiei adhaerere internae. Major praeterea adest macula multo magis opaca, orbicularis fere. Qui acutiori visu gaudet, eam nudis oculis uti punctulum albidum in ovo pellucido conspicit. Microscopium maculam a membranae superficie interna paululum distare lineola tenuissima demonstrat in eo situ conspicua, quo macula marginem contemplanti advertit (*Fig. V a.*). Tum et tenuitas hujus corpusculi apparet; discus enim potius est, quam tumulus. Si hicce discus ita situs est, ut faciem exteriorem advertat observatori, praeter orbem opacum ambiens halo tenuissimus nonnunquam percipitur. In *Fig. V b.* halonem rudi modo sculptum videtis. Tam subtilissima nemo nisi D'ALTON scalpro rite reddere valet.

Membranam internam saccum esse clausum vix crederes si collapsam contemplaris. Dilaceratione vero membranae corticalis de hac re certior factus sum, et nihil obstat, quin illa pro membrana vitelli habeatur, et discus opacus pro *blastodermate.*

In alia cane inveni ova minora, tertiam lineae partem diametro explentia, quae minus attente inspicientem effugere possunt facillime. Non ita pellucida erant, ac modo descripta, iis praeterea valde similia, si non forte strictius sphaerica. Eaedem membranae eodem modo disjunctae sunt sub microscopio; illa tamen granula in *corticalis membranae* superficie vix conspicua erant; maculae parvae contra in superficie interna sacci interni obscuriores sub microscopio valde amplificante uti acervuli e granulis minoribus congesti apparuerunt (*Fig. IV a.*); *blastoderma* crassius erat quam in antecedentibus, nec disci formam exhibuit, sed tumuli informis ac si nondum excultus esset discus.

In eodem utero ad tubae ostium granulum minimum, albedine vero conspicuum, libere natans inveni, quod microscopio subjectum globulum medium opacum cum halone vel peripheria lucida monstravit. Quid? si hoc corpusculum, licet sit opacum, ovum esset ex tuba modo delapsum? si globulus opacus *vitellum* vel *saccum intestinalem* futurum et peripheria *membranam corticalem* sisteret?

His igitur cogitationibus impulsus canem mihi comparandi operam dedi, cujus ova in tubis adhuc retenta essent. Quod bene successit. Canem enim emi, in quo corpora lutea valde hiantia, nullum ovum in utero, in tubis vero corpuscula albo-flavescentia inveni punctiformia, quae granulum modo memoratum ex asse aequabant, nisi quod hoc paululum majus visum est. Illa nunc fusius describam. Medium tenet globulus sub microscopio penitus opacus, superficie non laevi et aequali sed granulosa, totus enim globulus e granulis constat dense stipatis, membrana cingente vix conspicua (*Fig. III* °.). Globulum circumdat, interjacente spatio pellucido arcto, peripheria quaedam, stratu tenui granulorum minimorum obtecta. Post nycthemerae macerationem hujus pulveris majorem partem sejunctam inveni, quo facto membrana continua et simplex venit in lucem. Habemus ergo *membranam corticalem* et globulum internum, qui num omni careat cavitate me experimentis eruere non tentasse magnopere nunc doleo; — solidus quidem visus est, quod vero a sequentibus refutatur. Quem globulum *vesiculae intestinalis* vestigium, vel potius vitellum esse, jam enarrata suadent.

Mira est ovorum nostrorum parvitas. Quae sub microscopio metitus sum, $\frac{1}{17}$ lineae partem tantum diametro explebant. Nihilo tamen minus ob albedinem oculo nudo bene percipiuntur, quando sunt in conspectu. Facillime vero a tubae rugis et interjacentibus valleculis velantur. Sic facile fateor, me tria tantum invenisse, quamvis ex corporum luteorum numero sex exspectarem. Frustraneam tamen operam multorum anatomicorum in ovis intra tubas quaerendis eo tantum explicare possum, quod eos ova pellucida quaesivisse suspicer et inde vera neglexisse. Num vero in omnibus mammalibus ova in tubis sint opaca, nescio. Ova suilla in tubis hucusque non vidi ob majorem tenuitatem. Cuniculorum ova in

tubis non quaesivi. CRUIKSHANK admodum mancam de iis dedit descriptionem: „*The amnion* (*vesicula intestinalis*) *made a centre spot*“ (*Philos. Transactions for the Year* 1797. *pag.* 208); pingit tamen ita, ac si essent pellucida, unde vero nihil concludi potest, nam PRÉVOST et DUMAS ova canina in tubis viderunt, certe opaca, de quo tamen observatores tacent.

§. 3.

Ovula in ovario canino.

Restabat ut de ovorum conditione in ovario certiorem me facerem; nam ova tam parva vesiculas Graafianas ipsas ex ovario expulsas non sistere luce clarius visum est, nec verisimile habui tam solida corpuscula in tubis ex vesicularum fluiditate modo coagulata esse. Ovaria contemplans jam ante omnem incisionem in quacunque fere vesicula punctum luteo-album clare distinxi, quod velamentis vesiculae nullo modo affixum libere liquori innatare pressio, specillo in vesiculam facta, manifeste docuit. Curiositate quadam potius seductus, quam spe motus me nudis oculis per omnes vesicularum Graafianarum tunicas ovula in ovariis vidisse, vesiculam aperui, de quo dixi punctum cultelli lamina (tam distincte illud vidi et a muco circumdante discrevi) arripui et microscopio subjeci. Obstupui profecto, cum ovulum ex tubis jam cognitum tam clare viderem, ut coecus vix negaret. Mirum sane est et inexspectatum, rem tam pertinaciter quaesitam, ad nauseam usque in quocunque compendio physiologico uti inextricabilem tractatam, tam facillimo negotio ante oculos poni posse.

Ne longo sermone VOS, VIRI EXCELLENTISSIMI, teneam, ex multorum ovariorum caninorum observatione posthac instituta id tantum afferre lubet, quod ne ullum quidem inter ea erat, in quo non saltem nonnulla ovula, intactis ovariis, nudis oculis translucere viderim. In canibus obesioribus pinguedo etiam ovariis non penitus deest. In his pauca ergo ovula et haecce ab acuto oculo talibusque observationibus assueto ante dissectio-

nem percipiuntur. Idem fere valet de ovariis nonnullis, quorum velamenta nescio cur solito crassiora s t, uti in cane quadam magna observavi. In junioribus tandem canibus prima vice praegnantibus etiam pauca animadvertuntur ovula; nam post primum coitum, ni fallor, praeter ovula fecundata, reliqua si non nasci, citius tamen evolvi videntur, unde fortasse famosae illae observationes Anglorum HOME et GILES intelligendae sunt, fetus serioris praegnationis nonnunquam patrem primae sobolis ejusdem matris referre. (Cf. uberiorem disquisitionem BURDACHII in opere: *Die Physiologie als Erfahrungswissenschaft, Bd. I. pag.* 506.). In plurimis vero ovariis caninis complura ovula nudis quin etiam mediocribus oculis per tunicas apparent, uti nonnullis ea demonstrans commilitonibus expertus sum. Adhibita lente vitrea in omnibus fere vesiculis, minimis vix exceptis, ovulum percipitur, si canis non prima vice fecundata est.

Ovula ex ovario promta ovis tubariis consimilia esse jam dixi; centrum enim habent opacum granulosum cum halone. Ea tamen intercedit differentia, quod illa limbo lato circumdata sint, qui augente vi microscopii e granulis semipellucidis conflatus apparet, atque tam arcte cum ovulo connatus est, ut semel modo *membranae* externae (*corticalis*) aliquam partem prorsus denudare possem (*Fig. II.*). Peripherica illa congeries granulosa non est globosa sed disciformis, uti acu tenuissimo eam sub microscopio vertendo didici. Planetae Saturni annulum ita refert sub microscopio, ut BURDACH eam mecum observans hanc similitudinem iisdem fere verbis observaret, quibus jam diario notaveram.

Ovula in vesiculis caninis magnitudine secundum evolutionem variant. Majores (sine dubio aetate provectiores) diametrum $^1/_{13}$ — $^1/_{15}$ lineae Parisianae habent, minores, quorum centrum minus opacum est, vix $^1/_{18}$ lineae partem. Lamina annularis, quam *discum proligerum* nominabo, in omnibus adest, in junioribus tenuior, diametro ovulum bis circiter superante, in provectioribus obscurior, non semper eadem ratione ambitu suo crescens. Media pars, quae ovulo arcte conjuncta est, manifeste crassior existit ovulumque ita cingit, uti limbus artificialis lentem vitream, si non, quod magis adhuc arridet, ovi superficiem inferiorem penitus obducit. Ita *cumulo*,

de quo mox loquemur, canes sine dubio non carent, licet non tam promineat quam in plurimis mammalibus. Ne autem, quae in canibus facilius inveniuntur, his propria credamus, aliorum mammalium ovaria perlustrabimur omniaque, levibus varietatibus exceptis, congruere inveniemus.

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§. 4.

Vesiculae Graafianae quomodo constructae sint, et de ovulo mammalium generatim.

Vesiculae Graafianae in canibus nimis parvae sunt, quam quae cum magno successu examinari possint. In suibus vero saepissime et accuratissime, credo, eas investigavi. Cum his vaccas, oves, canes, cuniculos, erinaceum, delphinum, phocaenam nec non hominem comparavi, in omnibusque eandem constare structuram persuasum mihi habeo, quamvis in minoribus non omnia strata ita bene separare contigerit ac in sue, vacca et delphino.

Vesiculas Graafianas ovariis plus minusve insitas esse prout major minorve telae formativae vel sic dictae cellulosae copia adest, easdemque tempore oestri venerei et post conceptionem magis turgere, ex ovario emergere et demum aperiri, inter omnes constat.

Cum ea ovarii pars, quae vesiculam circumdat, hujus vi in involucrum mutetur partem vesiculae tegens, in describenda vesicula ovarium negligi non licet.

Ad quamque enim vesiculam Graafianam emergentem duae pertinent partes, pars includens vel *putamen*, et pars inclusa vel *nucleus*. Putamen constituitur partibus adscitis i. e. non vesiculis propriis sed ad ovarium ipsum pertinentibus, per vesiculam tantum sublatis et plus minusve in *indusion* mutatis, et *thèca* quadam vesiculae propria.

A. *Indusium* prominentem tantum partem vesiculae vestit et constituitur:

1. *Epithelio peritoneali* (cf. *Fig. IX.* 1.);
2. *Tela sic dicta cellulosa*, quae magis stipata est ad vesiculae

partem eminentem et totam ovarii superficiem, atque *albuginea* apud anatomicos nonnullos audit, laxior multisque vasis perreptata ad vesiculae partem immersam. Cum inter albugineam et parenchyma ovarii vix limites bene definitos observaverim, et illa in plurimis mammalibus tenerrimum efficiat stratum, diversis numeris in icone signare nolui. Praeterea numeros in icone *IX.* cum iisdem in hac expositione congruere jam elucet. Supervacaneum igitur esse duxi, figuram semper citare.

B. *Theca* duobus stratis componitur:

3. *Strato externo* tenui sed tenace, semipellucido, ex densa tela cellulosa conflato uti membranae tenues solent. Vasa recipit, in se continet et eorum fines in stratum sequens mittit.

4. *Strato interno*, crassiore, molliore, magis opaco, cujus superficies interna subtilissime granuloso-villosa et lubrica est. Superficies externa arctissime cum strato externo cohaeret. Laminarum separatio inde in minoribus difficillima est, in majoribus vero uti in suum vesiculis jam ante turgescentiam, inprimis post brevem macerationem cultello effici potest et saepissime a me facta est, vel vesiculis plenis vel evacuatis. Ablatis enim parenchymate et indusio, stratum externum thecae in conspectu posui cum vasis suis, quo caute inciso et ablato, stratum internum manifeste diversae structurae colorisque minus albi apparuit. Rationem inter externum et internum uno verbo indicaturus eandem esse contendo atque inter membranas pituitosas et adjacentem telam cellulosam, quae apud veteres tunica nervea audit. Stratum internum revera membranis pituitosis, quod ad texturam attinet respondere, nullus dubito. Adsunt scrobiculi scrobiculis muciparis similes. Vasorum fines in stratum internum penetrantes cito in ramulos tenuissimos sub penicillorum fere forma solvuntur. —

Distinctio inter ambo strata thecae non levis momenti videtur, quia ea tantum corporis lutei originem nobis demonstrat.

Thecae ea pars, quae posthac rumpitur, jam aliquod tempus ante rupturam tenuior videtur, quod potissimum a strato interno pendere credo. Apparet enim in vesiculis scrofae turgentibus saepius macula quaedam perlucida marginibus albescentibus irregularibus quasi exesis circumdata

et limbo laciniato, quo apertura corporis lutei posthac cingitur, simillimis. Macula illa (*Fig. XV.* picta) in quacunque vesicula diversam formam gerens, oriri inde videtur, quod stratum internum opacum subito in hac regione tenue fiat. Non vero in omnibus ovis maturis tale *stigma* animadvertitur.

Theca cum indusio post rupturam et nucfei ejectionem *calycem* constituunt.

C. Ad *nucleum* pertinent:

5. *Membrana granulosa* humorem vesiculae Graafianae includens. Constat strato tenui granulorum. In junioribus multo crassior visa est et opacior; in adultioribus tenuior, membranosa, membranae vitelli avium similior. Nunquam vero tantae tenuitatis, pelluciditatis et laevitatis illam vidi ac haec esse solet in vitellis maturis avium. Si vesiculae Graafianae discissae contenta in vitrum excavatum effunduntur, microscopio semper hujus strati disrupti frustula invenies (*Fig. XI.*). Clarius idem videbis, si placet vesiculam Graafianam turgidam per diem vel binos dies macerationi subjicere. Melius tum a *theca* solvitur et jam saepius, intacto vesiculae indusio, membranam granulosam collapsam transparere vidi. Si vesicula ita macerata sectione transversa sub aqua finditur, hujus membranae bina dimidia integra fere reperies. Si vero ictu vel brevi sectione vesiculas laedis, membrana semper pressione thecae dilaceratur. Idem fit cum ovulum a tuba recipitur. Ubi enim ova juniora in tuba uterove vidi, semper hujus membranae frustula in his organis inveni. De superficie hujus membranae laevi infra demum loquturus sum.

6. *Humor* vesicula contentus uti omnia fluida organica, excrementitiis exceptis et ne iis quidem penitus, ex *fluido nativo* (quo Needii *Grundschleim* reddere conamur) et granulis innatis componitur. Praeterea globuli oleosi inveniuntur plus minusve in omnibus mammalibus, quae hunc in finem examinavi, nec feminae deficiunt humanae. Granula parva sunt irregularia hinc et inde in acervulos collecta, granulis vitellinariis simillima sed ratione ad laticem pauca. *Fluidum nativum* viscidum pellucidumque est, ex vesiculis turgidis manifeste flavescit, quod tamen a granulis fortasse pendet. Lymphae praeterea est simillimus sed lymphae

spissiori, nam major quantitas, qualem saepius ex pluribus vesiculis suillis valde turgidis collegi, placentam veram et magnam quidem secernit. Ovario coctioni subjecto humor vesicularum penitus indurescit in massam albumini cocto quam simillimam, quae disrupta eodem fere modo ac albumen coctum superficiem striatam exhibet. In spiritu vini utique durescit, sed potius in massam granuloso-floccosam. *Fluidum nativum* ergo albuminosum est.

7. Fluido innatat vel impositus est *discus* ille *proligerus*, de quo locuti sumus, et *cumulus* ex dense stipatis granulis conflati. Haec granula albedine tantum ab humoris granulis descripti semper differunt. Tum minora tum majora visa sunt; de differentia magnitudinis nihil statuere ob eam causam audeo. *Discus proligerus* haud ita raro halonibus tenuibus notatur. Maximum halonem in erinaceo vidi. Nullum animal mammale his partibus carere vesicularum Graafianarum discissio sub aqua et diligens humoris perlustratio sub lente vel microscopio docet. Attamen quomodo et quonam loco sitae sint, haud ita facile eruitur. Nam in sola cane translucere solet ovulum cum disco suo. In binis ovariis suillis a tubarum epistomio involutis (inter centum fere a me examinata) etiam distincte per thecam et indusium majorum vesicularum apparuerunt. In reliquis ovariis, licet alia nonnulla tubae amplexae erant, nunquam vidi nisi incisione. Cum vero jam minima thecae laesione humor prosiliat, *discus* hac methodo non in situ observatur. Coquendo vinique spiritu humorem coagulavi, sed *discus proligerus* disparuerat cum ovulo. Spiritu denique vini valde diluto humoris partem paululum spissavi et *discum* ei incumbentem vidi, ut *Fig. A.* pinxi. Locum tamen indicare non potui. In cane mobilem inveni, sed semper ad cacumen magis quam ad fundum vesiculae observavi. In supra dictis ovariis porcinis immobilis erat et supremum locum in quacumque vesicula tenuit, quo differt a strato avium proligero ut plurimum sub pedicello sito. Cum omnes canes, quas dissecui, praegnantes, vesiculae ergo immaturae et clausae fuerint, cum porro in maturioribus vesiculis aliorum animalium *discum proligerum* nonnunquam *membranae granulosae* appressum invenerim, inde colligerem, *discum proligerum* a vesiculae maturitate magis magisque vehi ad peripheriam. —

Cumuli cacumen ad interiora semper versum est. Craterem nunquam distinxi. Cumuli forma est varia variis in animalibus atque in eodem animali manifeste variat, forsan secundum maturitatem; in cane depressus est; in vacca cylindricum fere (in ovis immaturis) (*Fig. XII.*) vel globosum vidi; in cuniculo sub iisdem formis, in femina depresso-hemisphaericum (*Fig. XIII.*), in porca saepissime sub diversissimis formis observavi. Quae ab eo fortasse pendent, quod ovulum mutet situm suum et haud minus quam discus proligerus a centro ad peripheriam tendeat *cumulum* et *discum proligerum* pedetentim penetrans. Ita suadent observationes in aliorum ovis animalium factae (v. §. 6.)

8. *Ovulum* tandem in *cumulo* et in ipso *disco* nonnunquam tale observatur quale de cane jam descripsimus. In reliquis animalibus vero non tam opacum vidi; qua ex causa, dum in ovario sunt, sine microscopio in his vix animadvertuntur. In omnibus massa globularis medium tenet obscurum et peripheria pellucida tum *membrana corticali* circumdatur. Ut constaret, num massa opaca centralis revera globulus sit solidus (talem enim habet adspectum), ovula canina et vaccae vel fortuito vel de industria dilacerata microscopio contemplatus sum, indeque globulum non esse solidum collegi sed granula ejus peripheriam crassam circa cavum minimum constituere (*Fig. II* *.). Res saltem ita est visa nec tamen facilis est dijudicatu. — Magnitudo denique ovulorum variat. Satis magna sunt ovula in porcis, vaccis, ovibus; minora in cuniculis; quibus minora adhuc in canibus, minima, ratione ad ovarium et totum corpus, in femina humana; eo minora ergo in universum quo altior corporis evolutio. In erinaceo tamen ovula et discus proligerus maxima visa sunt, cum vesiculas respicis Graafianas et corporis molem parvam. Denique afferam me bina ovula semel in canis vesicula distinctissime vidisse et alia vice bina ovula in scrofae vesicula vidisse me putare, unde numerus ovorum a corporum luteorum numero nonnunquam diversus facile explicandus est.

§. 5.

Historiae evolutionis mammalium brevis conspectus.

Ut, quae via retrograda investigavimus, clariora fiant, mammalium evolutionem a primo ovuli vestigio ad id usque tempus breviter persequamur, quo jam manifestior fit.

Quid prius adsit, utrum mammalium ovulum an vesicula Graafiana, num unquam oculis percipi possit humanis, nescio. Id vero certo certius habeo, in junioribus vesiculis *ovulum* cum *disco proligero* jam adesse nec ad vesiculam cui pertinet ita parvum. In minimis quidem non vidi, sed disquisitio haec tantis premitur difficultatibus, ut et in majoribus interdum frustra quaesiverim. Observationibus deficientibus, cum hypothesin substituere licet, ovula priora esse crediderim. Demonstrabo enim mammalium ovula vesiculis PURKINII reliquorum animalium comparandas esse, quas in animalibus nonnullis molluscis, acephalis v. c. et lumbricis, ovorum evolutionem antecedere clare me vidisse puto.

Vesiculae Graafianae humor primitus albumen inspissatum aequat dum solum ovulum granulosum est. Tum differentia polaris in humore magis magisque evolvitur, fluidum nativum dilutius fit et granula contra plura in eo evolvuntur.

Membrana, quam granulosam dixi, principio stratum satis crassum et continuum est, tum granula in ea magis excoluntur, quae externa superficie tenuissimo tegmine albuminoso tegi videntur, nunquam quidem ita separato ac in ovis avium, tamen membranae vitelli (avium) aemulo. *Discus proligerus* magis condensatur et ovulum pedetentim ab eo sejungitur *membrana corticali*, quam utique pro secretione ex ovuli globulo habeo. Uti enim quodcunque granulum peripheriam habet pellucidam (in granulis membranae granulosae bene perceptilem et fortasse cum halone ab auctoribus circa globulos sanguinis observato comparandam), ita et quaeque granulorum congeries peripheria communi se circumdat. In ovulo cavitas centralis oritur, granulis ad peripheriam tendentibus. Omnis enim evolutio in eo nititur, quod a centro ad peripheriam tendat formatio. Etiam discum proligerum et ovulum ad peripheriam progrediente evolutione vehi,

observationes in mammalibus si nondum stricte probant, suadet tamen comparatio cum ovis reliquorum animalium. In vesiculis maturis denique stratum internum *thecae* crassius fit, *stigma* vero tenuius demum evadit.

De *corporis lutei* genesi satis dissentiunt observatores. Me judice, minime corpus novum est, sed stratum internum thecae magis evolutum. Quod sequentibus observationibus demonstrari posse puto.

1. Circa corporis lutei partem immersam unum modo involucrum discernere potui, quod *thecae* strato externo respondet. Ex eadem enim tela constituitur nunc paululum distensa, uti facile in magnis vaccarum, ovium, porcarum corporibus luteis probatur. Nec reliquae circa corporis lutei vestimenta disquisitiones dissuadent.

2. Apertura corporis lutei nondum confecti idem docet; cingitur enim *peristomio* laciniato. Has lacinias minime esse *indusii* prolongationes, sed ipsius corporis lutei, distinctissime vidi paulo post rupturam in ovariis caninis, cuniculinis et porcinis. *Indusii* apertura enim major est cingitque peristomium (*Fig. XIV.*). Peristomium vero eandem habet formam ac stigma supra descriptum nec quidquam aliud est.

3. Expulso vesiculae Graafianae *nucleo* fluidi albuminosi aliquid remanet, ex quo vero nunquam corporis lutei massam augeri inde concludo, quod 1) superficies hujusce interna semper definite circumscripta sit in quacunque protuberantia; 2) saepissime in suibus corpora lutea per totam graviditatem cava et fluido spisso albuminoso impleta inveniantur, dum alia sunt solida. Hoc inde pendere videtur, quod in aliis corporis lutei excrescentiae (protuberantiae) internae cavitatem compleant et fluidum propellant antequam laciniae peristomii concreverint, in aliis laciniae concrescant ante corporis soliditatem lutei. Nunquam enim apertura clausa humor in corpus luteum inclusus mutatur. In canibus et ruminantibus corpora lutea, si clausa erant, cava me invenisse non memini: in his apertura primitus major est quam in suibus.

4. Corpus luteum statim post ovuli ejectionem adest. In ea cane, quae ova ex tubis suppeditaverat, corpus luteum inveni cavum, late apertum, non valde crassum, superficie externa rugatum (quam superficiem corpora ruminantium lutea servare solent, unde vana illa compara-

tio cum glandulae acinis), interna multis eminentiis, quas cum rugis et villis componerem crassissimis, obsita. Eminentiae rubebant vasis amplis, jam nudis oculis sine injectione apparentibus. Doleo, me aliis investigationibus detentum haec corpora lutea non delineavisse. Aliud paulo provectius *Fig. XIV.* exhibui lectoribus.

5. Sed jam ante *nuclei* ejectionem thecae stratum internum metamorphosin in corpus luteum subire persuasum mihi habeo. In cuniculo quodam, cujus ova in utero jam jacebant, nescio quo casu, una vesicula valde turgens adhuc clausa erat. Apertam eam nondum fuisse ovuli praesentia demonstravit. Corpus luteum certe tamen adfuit quamquam tenue et sine excrescentiis internis. Idem in vesiculis turgidis porcarum me observasse puto, in quibus stratum internum thecae incrassatum et paululum ex luteo rubens inveni. Corpus luteum in suibus enim nunquam vere luteum est. Ante stigmatis aperturam lutescit quidem, sed post nuclei ejectionem rubrum fit. Eundem colorem in canibus habet; in vacca luteo-aurantiacum est; in homine in primordio flavum. Quemnam colorem posthac in femina humana habeat, non satis expertus sum in ovariis recentibus. Persuasum ergo habeo, jam turgente vesicula Graafiana stratum *thecae* internum incrassari et, disrupto indusio, a strato externo et ambiente tela cellulosa nunc se contrahentibus crispari, unde rugae jam nascuntur internae, quae tamen subito majores excrescentias propellunt villis maximis similes, et postremo inter se confluunt. Ex quolibet arteriarum fine in stratum internum penetrante nunc reticulum parvum excrescentiae cuique nascitur.

6. Haud minus in vesiculis porcinis morbose multo humore (nonnunquam ad ovi gallinacei minoris ambitum) distensis *thecae* stratum internum corporis lutei indolem induit. Crassitiem semilineae assecutum vidi: num fortasse corpora lutea hydropica nec vesiculae morbosae?

7. Quibus disquisitionibus ut finem imponam, afferam tandem me in puella libidinosa thecae stratum internum vesiculae adhuc clausae jam omnino flavum vidisse. In alia puella, quae morti se dederat, procul dubio ob inchoatam graviditatem corpus luteum inveni apertum. Cujus cavitas, ratione ad vesiculam habita, major erat quam in ullo corpore

luteo alterius animalis mihi obvio, et protuberantiae internae minores. Unde, quaeso, haec differentia, nisi ex eo pendet, quod vesicula Graafiana humana a densiore tela cellulosa circumdata minus contrahatur post evacuationem? Ovum in hoc casu rarissime se offerenti non inveni, quod quidem tunc temporis proh dolor! ovum minimum mammalium nondum cognoveram.

Ad ovum redeamus. Ex ovario ejectum a tubae apertura abdominali recipitur. Quod ut certius et melius fiat, haec apertura non tantum sub infundibuli forma extenditur, sed et alia adsunt adminicula. In animalibus nonnullis, in quibus ala vespertilionis anatomicorum vix excavata sed potius plana est; infundibuli ostium peristomio laciniato cingitur vel sic dictis fimbriis; ita in homine, simia. In aliis ala vespertilionis anteriora versus in bullam extensa est, quo fit, ut ostium tubae ovario sit propius. In his ostium jam magis apertum fimbriis caret veris. Exempla habemus in ruminantibus. Sunt porro alia animalia, in quibus bulla dicta sit profundior et posteriora versus ad aperturam quandam contracta. Ad haec pertinent sus et delphinus. Tubae infundibulum ad bullae aperturam aperitur nullis, me judice, fimbriis, sed rugis obsitum et ostio magno in bullam transit. Tempore oestri venerei bulla descripta ovarium ut mitra obducit, quod in scrofis saepius, in phocaena semel vidi. Apertura tam ampla est, ut bulla ab ovario sine laesione distrahi possit. In insectivoris bulla jam in saccum mutatur, ovarium uti tunica vaginalis semper includens, apertura coaretata quidem sed satis conspicua. In feris jam ita contrahitur, ut aditus primo intuitu praetermitti possit; in cane rima angusta, in phoca foramen parvum adest. In mustelas inquirendi nunc deest occasio.

Cum ovulo semper membranae granulosae frustula in tubas transeunt: nam in utero haud minus quam in tubis cum ovis primitivis frustula dicta semper adsunt. Praeterea in his organis inveniuntur vesiculae oleosae, granula polymorpha albuminis et frustula telae cellulosae per cultrum vel forficem abscisa. Quas omnes res observator cautus cum unquam viderit, ova minora cum iis non confundet. Quomodo vero hydatides pro ovis haberi possint, uti Prévost et Dumas suspicantur, equi-

dem non intelligo. Hydatides enim licet catenas in suibus viderim, nunquam tamen in tubarum aut uteri cavitate offendi. Corpuscula aliena, quantum potui, *Fig. VIII.* pinxi; pleniorem descriptionem supervacaneam esse duco. Iter per oviductum faciens ovum mammalium nullam fere subit metamorphosin nisi quod mucum albuminoso-gelatinosum imbibat, inde paululum crescat et ita quidem, ut granula globuli interni peripheriam magis magisque constituant humorem includentem. Granula cum humore *citellum* sistunt, cujus cuticula globulum obtegens ex granulis secreta vix percipitur, *membrana corticali* jam bene exculta.

In uterum transmissum ovum citius accrescit majorem fluidi quantitatem imbibens et inde cavum et pellucidum fit. Granula ad peripheriam magis magisque recedunt et ex superficie sua materiam excernunt, ex qua cuticula tenuissima concrescit, cujus superficiei internae granula adhaerent. Cuticula haec membrana vitelli est, et granula, de quibus agimus, granulis vitellinis avium respondent. Villorum vestigia in *membrana corticali* apparent dum in utero ovum libere movetur. Vis formativa a centro ad peripheriam agens, cum granula primitus ad peripheriam egerit, tum in quocunque granulo massam densiorem ad peripheriam cogit, quo quodque granulum in orbem vertitur, granula minora circa centrum pellucidum sistentem.

Metamorphosis ovi ergo vesiculae Graafianae metamorphosi omnino respondet. Sicut in hac granula plurima stratum peripheriam constituant, ad cujus superficiem laevis membranula secerni incipit, ita et in formanda membrana fit vitellinali. Porro, in vesicula Graafiana stratum proligerum cum cumulo adest, cui respondet blastodermatis historia. Pars granulorum enim in ovo sub membrana vitellinali tumulum constituit, pedetentim in discum mutandum. Membrana corticalis *theeae* vices gerit.

Ovum ante fetus praesentiam lente crescere mihi visum est. Fetus eodem modo evolvitur, quo in avibus, nec situs in axi transversa ovi differt. Primitus spinae dorsalis vestigium apparere videtur. Ex hoc laminae dorsales (*plicae primitivae* PANDERI) et paulo post laminae ventrales excrescunt. In omnibus enim animalibus vertebratis evolutio (semper centrifuga) in eo nititur, quod ex toro centrali (columnae vertebralis

vestigio) binae laminae sursum tendant ad cavitatem pro medulla spinali et cerebro constituendam, et binae deorsum ad cavitates pro organis plasticis vasorumque truncis formandas. Quam evolutionem persecutus sum in batrachiis, ophidiis et sauriis haud minus quam in avibus. In piscibus primam quidem non vidi evolutionem, sed quantum ex pisciculis pellucidis trium linearum, in concharum branchiis semel obviis et breviter examinatis et ex majoribus octo ad duodecim lineas explentibus, in spiritu vini servatis colligere potui, eandem esse censeo, de quo clarissimus RATHKE exspectatissimo suo de piscium evolutione opere certiores nos faciet. Verum ad animalia vertebris carentia quod attinet, eorum evolutionem in articulatis a ventre ad tergum procedere jam ante RATHKII expositiones (*Isis* 1825 pag. 1098) cum BURDACH expertus sum et jam anno 1824 publicis praelectionibus docui, uti auditorum schedulae testabuntur. RATHKE hac de re a BURDACH monitus vere et ineunti aestate anni 1825 literis contradixit, auctumno autem ejusdem anni publice eam docuit, propriis sane observationibus subtilissimis edoctus *).

Dum embryo evolvitur, blastoderma haud dubie extensum et ampliatum cum membrana conèrescit vitelli, quo nunc verus saccus intestinalis constituitur, ut in avibus. Membranae corticalis villi multo magis propullulant et ovum ab utero, cujus villi utique excrescunt, arctissime cingitur. Substantiae nutritivae, quibus ovum avium jam ante partum prorsus instruitur in magna sua vitelli et albuminis quantitate, ex utero mammalium commercio villorum pedetentim ovo suppeditantur, quo fit ut saccus intestinalis in prima periodo crescat, reliquae autem membranae per totam graviditatem.

Sed haec sufficiant de mammalibus, ne repetam quae de concordia et differentia in mammalium avium que evolutione jamjam docui.

*) Quibus nunc publice enunciatis inter nos pax sit post levem stomachum, quem eàm tantum ob causam prae me tuli, quod jam eo tempore discrimen inter animalia vertebris carentia et vertebrata promulgarem, si non disquisitionibus brevibus quidem sed ad hoc discrimen dijudicandum sufficientibus fretus, speraverim, me hoc amici observationibus additurum esse.

§. 6.

Comparatio ovuli mammalium cum ovis reliquorum animalium.

Relata si cum iis comparamus, quae de animalium ovis hucusque nota erant; ova mammalium, ad genesin quod attinet, non differre tantum sed omnino peculiaria esse videntur. Mens humana vero nullo modo acquiescit in sola differentia inter res naturales ejusdem dignitatis et relationis cognoscenda. Jam dudum docuit morphologiae studium, omnem cujusvis organi (ne omnium dicamus organorum) differentiam inter limites majoris similitudinis teneri. Quid? Ova mammalium huic doctrinae a CASP. FRID. WOLFF tanta jam claritate demonstratae contradicerent!

Hisce cogitationibus commotus complurium animalium ova accuratius examinare constitui. Exercitationibus circa hanc rem facem praetulit excellentissima dissertatio, qua JOANNES EV. PURKINJE venerabili BLUMENBACH semisaecularia gratulatus est. Libellus, parvi quidem ambitus, sed gravissimi argumenti, publice, quantum scio, non venditur, sed mutuo academiarum commercio literario duntaxat distributus est, quo evenit, ut haud ante autumnum anni 1826 cum cognoverim. Titulum habet sequentem: *Joan. Fried. Blumenbachio etc. summorum in medicina honorum semisaecularia gratulatur ordo medicorum Vratislaviensium interprete Joanne Ev. Purkinje P. P. O. Subjectae sunt symbolae ad ovi avium historiam ante incubationem; cum duobus lithographis. Vratislaviae, typis universitatis* (anno 1825 mense Sept. edit.).

Quae oculatissimus PURKINJE observavit, sunt haec: In avium ovis, dum in ovario jacent, sub membrana vitelli stratum tenuissimum granulorum vitellinorum adest, quae uno loco magis stipata zonulam (quam *discum proligerum* nominavimus) constituunt. Zonae superficies internae cumulum mammaeformem eorundem granulorum ad interiora vergentem exhibet. In cacumine cumuli porus pellucidus, qui ab utraque disci pagina conspici potest, formae circularis integerrimae offertur, cujus diameter sextam fere lineae partem explet. Hunc porum, qui medium cicatriculae cumulum pertundere videtur, vesicula occupat minutissima, quae substantiae granulosae

immersa, duas tantum facies, alteram versus membranam vitelli, alteram per colliculi cacumen cratere ibi parvulo circumdatam, liberas praesentat, ita tamen, ut ibi aliquantulum membranae adhaereat, hic vero stratum simplex granulorum inter se cohaerentium per eam continuatur. Vesicula humorem diffluum continet limpidissimum. In cicatricula ovorum partorum et in utero vel in oviductu latentium nunquam invenitur, in vitellis autem, dum in ovario sunt, ne minimis quidem exceptis, semper adesse videtur, quare Cl. PURKINJE vesiculam dictam ovi primordia constituere suspicatur.

Quibus tamen liceat addere, me quoque in ovis minimis, ob granulorum defectum pellucidis, vesiculam quandam vidisse, et quidem, ni fallor, ad centrum magis positam quam posthac sita est. In ovis a $\frac{1}{4}$ lin. ad unius lineae diametrum excretis, obscurioribus et albescentibus, vesicula jam sine dissectione non apparet. Videmus quidem sub microscopio orbem in ovo satis magnum, qui tamen non a vesicula sed a vitelli membrana pendet per thecam transparente. Vitelli membrana hoc tempore crassissima et granulosa est ita, ut optime mereatur nomen *membranae granulosae*, quam PURKINJE quoque ita describit. Autumat vero granula superficiei internae membranae adhaerere; sed innata sunt, me judice, et membrana potius ex iis quodammodo constituitur; abstergi enim non possunt. Posthac *membrana granulosa* in stratum externum laeve, continuum — membranam vitelli puta — et stratum granulosum, vitelli peripheriam efficiens, sejungi mihi visa est. Quae qualicunque modo sese habeant, id quidem certum habeo, vesiculam tunc temporis non in strato granuloso jacere, sed sub eo locum habere eidemque ab interiori latere impressionem dare, ut ovulum mammalium.

Quae praeterea excellentissima docentur in opere dicto, huc non pertinent.

Vesiculam Purkinji (hoc nomen enim justo titulo geret illa, licet forsan jam ab alio observatore visa sit, cum auctor noster eam tam accuratis et subtilibus investigationibus persecutus sit, ut nunc demum nota et historia ejus in ovo gallinaceo fere absoluta videatur) in aliis ovis quaesivi, atque, nisi in iisdem relationibus, tamen in similibus, in omnibus inveni. His investigationibus multum temporis et operae navavi, eas vero

nondum absolutas esse optime scio, neque absolvi posse nisi plurium annorum labore credo.

Omnia, quae circa quodcumque ovorum genus vidi, dubitavi et revidi, hic penitus referre taediosum esset. Saepe enim est difficile dijudicatu, quodnam nomen pars aliqua mereatur. Ea igitur tantum exponam, quae mihi summam observationum conficere videntur, et quidem non ovorum genera secuturus, sed partes constitutivas. Ova mammalium nunc penitus negligamus.

In omnibus igitur ovis a me examinatis *vesicula Purkinji* adest ante evolutionem absolutam. In minoribus jam apparet; imo primum ovi vestigium esse videtur, circa quod vitellus colligitur. Mollusca circa hanc originem me fecerunt certiorem; in molluscis enim vesiculas pellucidas minimas ante ova vidi, tum vesiculas strato vitelli obtectas et postremo ova magis magisque evoluta. Idem, ni fallor, valet de Lumbrico nec non de Hirudine. In aliis ovis evolutis ita definite observari non potest, quoniam quidem *thecis* conduntur.

Vesicula *cumulo* innata est, ratione ad vesiculam admodum variante. In aliis enim maximus, uti in molluscis, in aliis minor, ubi vesicula magis exculta est, uti in sauriis et ophidiis. *Cumulus* praeterea in animalibus inferiorum classium non ita manifeste granulosus est quam in superiorum: in molluscis albumini inspissato fere par est. In insectis cumulum hucusque discernere non potui; idem de piscibus confiteor; in his enim vesicula satis magna a globulis oleosis stratum quoddam conficientibus cito obvelatur. Quod stratum num discum referat cumulumque nescio. In batrachiis, quibus utique vesicula est maxima, distincte appareret circa eam massa peculiaris, quam pro cumulo habeo.

Nec vesicula praeter magnitudinem ejusdem indolis est. In ovis junioribus omnium animalium pellucida est, fluido nunc nulla nunc minutissima granula fovente impleta. In molluscis, annelidibus, insectis, crustaceis (Astaco fluv.), batrachiis et avibus in hoc statu remanere videtur, quamvis in nonnullis ad maturitatem jam granulorum copia augeatur. In ophidiis et sauriis vero granula fluidi inclusi majora et frequentiora apparent, tum lutescunt et ad maturitatem tandem saccum internum con-

stituunt, vesiculae membranae adjacentem; in ovis maturis saccus internus granulosus a membranula vesicae externa pedetentim recedit et collabitur, si vesicula aquae submersa est; de quo conf. *Figuras XVII.* et *XVIII.*, *XXI.* et *XXII.*

In omnibus animalibus, ni fallor, vesicula a medio ad peripheriam recedit, quae res in batrachiis optime observatur. In ranis enim bufonibus, salamandris compluribus vesiculam in ovis immaturis a membrana vitelli satis distantem inveni. Idemque de avibus jam docui, ubi vero multo citius in superficie apparet. Diutius in lacertis remanet, ita ut in ovis jam dudum luteis punctum pellucidum (vesicula scilicet) non animadvertatur. Talia ova sub aqua cum caute secantur, distincte invenitur stratum vitellinarium, cui *vesicula Purkinji* jam interiori lateri facit impressionem (*Fig. XIX.*). Postea sub membrana vitelli jacet, strato granuloso perforato, quod luce clarius vidi in ranarum ovis, ubi imo vesicula magna membranam vitelli in colliculum elevat. Haud minus in Anodontis vesiculam cum cumulo e vitello protuberare vidi.

In ovis partu jam editis nunquam inveni vesiculam. (Piscium ova parta examinare hucusque datum non est). Jam in oviductu non invenitur. Nihilominus cum PURKINJE non facio, qui oviductus pressione vesiculam disrumpi credit. Semel enim in ovo gallinaceo maturo, in ovario quidem retento sed ad ejectionem parato, vesiculam non inveni. Quam unicam observationem multi non facio, cum facillimum sit in errorem labi. Majoris mihi videtur momenti, quod in teneris serpentum oviductibus ova non minus vesicula carere visa sint, et quod vesiculam protuberantem jam in ovis ranarum ovario adhuc retentis observaverim. Denique ova in ovariis insectorum matura vesicula carent, immatura vero in ovariorum apicibus nidulantia eadem praedita sunt. Num forsan fecundatione vesicula dissolvitur? Id quidem non credo; in batrachiis enim ova in oviductu vecta vesicula carent longe ante fecundationem; in insectis perfectis fecundatio in alterno fine apparatus genitalis fieri videtur, ova autem jam omnia, supremis et vix apparentibus tantum exceptis, vesicula carent. Imo in nymphis ova majora et ad maturitatem magis provecta, certe vero nondum fecundata, carent vesicula, quae tamen in minoribus

adest atque superioribus. In ovis denique, quae gallinae sine gallo viventes enixae sunt, vesicula deest, et jam in oviductu deerat, ut PURKINJE jamjam observavit atque ego expertus sum. Inde vesiculam colligo ex ovo maturo protrudi et dissolvi inter vitellum vitellique membranam ante fecundationem. Nusquam tamen membranam vitelli perforat. Docent quidem PRÉVOST et DUMAS, ova ranarum recens edita porum habere in membranis. Contrariam ego quidem habeo sententiam. Maculam enim luteam in summo vertice ovorum ranae saepius obviam ea subtilitate et pertinacitate examinavi, qualem rei difficultas tantorumque virorum gravitas requirunt, at foramen non inveni in vitelli membrana. Punctulum obscurum in macula lutea ab fovea angusta et profunda in ipso vitello pendet. Stratum praeterea granulorum nigrum foramine pertusum esse vidi (*Fig. XXVI* ᵇ.) et vitellum cum materia aliqua minus granulosa (*nuclei* scilicet) supra margines strati dicti eminere, ut haec omnia *Fig. XXVI* ᵃ. pinxi. Itaque stratum nigrum ab interiore facie ad exteriorem, illaesa vitelli membrana, pertusum esse clare apparuit. Haec mense Aprili in Rana temporaria visa et picta optime congruunt cum vesiculae protuberantia mense Majo exeunte in Rana esculenta observata et magna caverna in iis ovis, in quibus vesicula modo disparuerat. Nec PURKINJE, qui in avium ovis ex vesicula dissoluta nasci conjicit sic dictum colliquamentum, sequi queo, cum quidem in ovo maturo vesicula certe ex strato granuloso jamjam promineat. Praeterea, num revera in oviductu jam colliquamento gaudent ova gallinacea? — Liceat igitur mihi quoque proponere hypothesin, ab ulterioribus disquisitionibus vel in dogma sanciendam vel cum aliis refutandam. *Vesiculam Purkinji* partem ovi efficacem esse credo, qua facultas feminina vim exerceat, ut facultas masculina semini inest virili. Vesiculae igitur protrusio et dissolutio ab ovi maturitate et forsan irritatione penderent. Post fecundationem verum blastoderma eo loco evolvitur, quo vesiculae humor effusus est. Macula enim in ovis gallinaceis obvia dum in ovario continentur, blastodermatis nomen non meretur, sed ejus prodromus potius videtur. Vitello paululum immersa est et ambitu non gaudet definite circumscripto. In omnibus, ni fallor, ovis stratum quoddam granulosum ante blastodermatis evolutionem adest.

vel sub disci, vel sub lamina magis extensae et curvatae forma, semper vero disci proligeri mammalium, de quo diximus, locum tenens. Minora Astaci fluviatilis ova saepenumero magnopere complanata sunt, quoniam quidem nihil fere continent, nisi stratum granulosum cum vesicula et cumulo (*Fig. XXIII.*). In ophidiis et sauriis talem discum in ovis ad summam pene maturitatem provectis, maxime excultum inveni halonibusque plus minusve notatum. Granula enim in circulos ita congesta erant, ut in aliis — magis luteis — granula praevalerent, in aliis — magis obscuris — latex granula conjungens (*Fig. XVI.*). Blastoderma e contrario in oviductu serpentum post conceptionem et vesiculam disruptam crassius reperi, supra vitellum elevatum facileque auferendum, licet marginibus cum vitello concretum adhuc esset, nullis halonibus primitus notatum, imo oblongum: cum quidem stratum, quod cum disco proligero compono, orbiculare esset. In batrachiis, acephalis, gasteropodibus discum proligerum circa vesiculam non vidi; in his etenim pro disco immerso granulosum stratum circa vitelli partem majorem bene percipitur. In batrachiis nigrum est ovique dimidium amplectitur. Id, postquam vesicula perforatum est, crassius fit multumque in ambitu crescit, jam blastodermatis vices gerens. Discus vel stratum proligerum in omnibus forsitan ovis aliquid ad blastoderma constituendum confert; ubi vero primitus magis a membrana vitelli distat, non tam directe in blastoderma mutatur. Sic in Astaco fluviatili exeunte autumno discum proligerum in ovis majoribus haud minus discrevi, quam in minimis post annos deponendis. Mense Aprili vero, ubi ova jam parta sub cauda inveniuntur, primitus blastoderma adest nullum, tum sub forma nebulae indeterminatae apparet (*Fig. XXX.*); mox solidescit et limites distinctos exhibet, quasi disci proligeri substantia sensim semsimque huc migraret. Simile quid Herold in aranearum ovis observasse videtur, cum de macula loquatur dissoluta et denuo formata.

Atque ad ovorum conditionem quod attinet, haec duntaxat monebo. Ova gignuntur in ovariorum parenchymate, quod in superioribus animalibus, generatim sumtis, densius esse solet quam in inferioribus, in quibus ad statum liquidum fere nonnunquam accedit. Cum rebus comparandis

semper claritatem afferat, eandem rem, quacumque sub forma apparet, eodem nomine signare, voce uti velim, quae ubique adhibenda sit. Vox „sarcocarpium", a RICHARD pro parenchymate pericarpii (et inde etiam germinis) plantarum proposita in animalia non satis quadrare videtur; aliam ergo de mycophilis desumam, *stroma* (*Lager*) scilicet. *Stroma* cum ovarii *indusio* id constituit, quod BURDACH in omnibus organis proliferis, vel ova foveant vel sporas, *matricem* nominat. Ova *stromati* vel innata sunt, ut in pluribus animalibus; vel imposita, ut in Hirudinibus, quarum *stroma* singularem filorum crassiorum formam habet, distante *indusio*. Ova praeterea vel *thecis* continentur (in plurimis animalibus), vel iis carent (in annelidibus, acanthocephalis, trematodibus aliisque). *Thecae* denique vel ovo cuique sunt *propriae*, quae res, ni fallor, de animalibus vertebratis omnibus valet, pluribusve ovis *communes*. *Thecae communes* praeter ova massam continent *stromatis* vices pro singulis ovis gerentem, et *sporangiis* botanicorum aequiparandae sunt. Inclusum illud stroma sub *stromatis interni* nomine a *stromate extra thecas* posito discerno. *Thecae communes*, quantum scio, nunquam cum ovarii indusio concretae sunt, verum partu ejiciuntur cum stromate interno. In acephalis tales *thecae communes* manifestae apparent. Quid tamen de insectis sentiam, dubius adhuc sum: Ovaria igitur ita dicta pro meris *thecis* sumerem *communibus*, sic ut stroma externum omnino deesset, et materia pulposa inter ova sub variis formis (modo clavas constituit inter ova sitas, modo corpora utrinque excavata, piscium vertebris similibus, modo omnino deesse videtur) observanda *stromatis interni* nomen mereretur. Haec suadet ovariorum connexus cum vase dorsali vel corde, nec non indusium, quo ovaria in erucis circumdantur. Quod si a vero non recedere videtur, quid de naemotoideis censendum est? In acanthocephalis ova massae semifluidae innata sunt, quae, cum in ovarii cavitate sit, ad *stroma internum* insectorum accedit, *thecis* vero communibus non includitur.

Sed haec sufficiant ad ovaria mammalium comparanda eorumque contenta cum ovario et ovis reliquorum animalium. *Stroma* habemus in mammalibus stipatum. Cui stromati innatae sunt vesiculae Graafianae, quae cum reliquorum animalium ovis comparatae miram exhibent concor-

diam, cum ad partes constituentes tantum respicimus. Conferamus v. c. mammalis cujusdam ovarium cum avis ovario et ranae. Ovarii indusium, stroma, thecae adsunt, ea tantum differentia, quod indusium et stroma in avibus ranisque tenuiora sint, quod in ranis praeterea indusium internum propter internas cavitates appareat tenuissimum, quodque tandem theca in ranis ad interiora aperiatur et calycem interiorem bene distinguendum efficiat. Venit tum vitelli membrana, quae in mammalibus adest sub membranae forma granulosae. Cum membrana vitelli in avibus et reptilibus primitus sub membranae forma appareat granulosae et ex hac sejungatur, dubius non haereo vesiculam mammalium Graafianam circa hanc partem cum ovo reliquorum animalium vertebratorum immaturo componere. Vitellus avium et ranarum e granulis permultis vitellinis minorique quantitate albuminis fluidi constat. Vesiculae Graafianae eo duntaxat differunt, quod minorem habeant granulorum quantitatem, majorem contra fluidi nativi albuminosi. Praeterea ovis similes sunt. Habent vesiculam enim inclusam (ovulum scilicet in mammalibus) cumulo immersam stratumque proligerum nunc vesiculam cingens nunc ei superjacens. *Vesicula ergo Graafiana cum ad ovarium generatimque ad corpus maternum respiciamus, ovum sane est mammalium.* Sed evolutionem quod attinet, vehementer discrepat a reliquorum ovo animalium, quorum ovi nucleus integer ex ovario devehitur, fetui nascituro non sedem tantum praebiturus sed in ipsum potius fetum transformandus. In mammalibus vero *vesicula innata* vitellum magis excultum continet et *ratione ad fetum geniturum habita verum sese probat ovum. Ovum fetale dici possit in ovo materno.* Mammalia ergo habent ovum in ovo aut, si hac dicendi formula uti licet, ovum in secunda potentia. *)

Quam differentiam ut melius concipiamus, ad saurios redeamus et ophidios, quorum ovum ad mammalium ovum propius accedit, quandoquidem *vesicula Purkinji* ad maturitatem granulorum vitellinorum haud

*) Quapropter in vesicula Graafiana describenda §. 4. voce *ovuli* semper usus sum, quia vesicula Graafiana ipsa ovum refert, respecto ovario, ex ovulo autem fit ovum fetale.

parvam continet copiam in sacculum internum demum coalescentium. Unde haec concordia! Lacertarum et serpentum ova in oviductu saepius examinavi et fetum in oviductu per plures hebdomades evolvi didici, et non deponi, nisi allantoide jam bene exculta ad respirationemque in se recipiendam apta, serpentibus aliquot et Lacerta crocea exceptis, fetum usque ad ovi velamentorum rupturam retinentibus. *) Ex quo concludo: *quo diutius in corpore materno fetus foventur, eo magis jam primitus exculta videtur ovi vesicula innata, quae in mammalibus eo pervenit ut omnes ovi virtutes in sese recipiat et reliquae ovi partes parvi momenti extraneae quasi fiant.* Quae sententia ex observationibus hausta mirifice sane cum hypothesi nostra congruit, *vesiculam Purkinji* partem esse efficacem sexus sequioris; nam quo major vis matris in fetum, eo magis exculta esse debet pars ovi efficax, eoque diuturnior pulli sedes in matris corpore. Crediderim fere commorationem fetus a matris potius vi et inde ab indole ovi pendere, quam a fetus necessitate. Nam ovis ex oviductu Lacertarum promtis fetus mimime moritur.

Finem jamjam assecutus quandam solvere lubet dubitationem. Quaerent enim fortasse nonnulli: quid? cum vesiculae Graafianae ova reliquorum animalium aequant, quae est causa, cur in mammalibus duntaxat corpus luteum oriatur? Quibus hoc ab ovarii stromate et thecae indole pendere respondeo. Illud enim in solis mammalibus tantae est densitatis, ut thecae cito collabi nequeant. Atque thecas quod attinet, in avibus, quas exempli gratia eligimus, microscopio adhibito, puncta in strato interno exhibent pellucida, quae vel pervia vasorumque fines sunt, vel cuticula tenui tantum teguntur. Quibus igitur punctis secretio facillime efficitur. In mammalibus lucidiora quidem non desunt puncta; minime tamen perforata, verum ita constituta, quales sunt cryptae muciparae in membranis mucosis, quantum anatomicis innotuerunt: contra in mammalibus vasa, in uterum

*) Lacerta crocea Wolffii certo vivipara est. Ejusdem fetus jam fere confectos ex oviductu excidi. A Lacerta agili, non obstante magna verborum farragine a Ferd. Schultze in *Verzeichniss der Doubletten des zoologischen Museums zu Berlin*, 1823, p. 93 seq. prolata, toto coelo diversa est. Differt non tantum colore et magnitudine, sed poris femoralibus, collaris et squamarum structura.

hiant, ubi humorum nutritiorum major quam in thecas affluxus est. Etenim talia ostia nec ita parva in aliquot uteris vidi distinctissime. Itaque in mammalibus secretionem in theca eo modo perfici persuasum habeo, quem DOELLINGER exposuit, i. e. praeeunte organi secernentis nutritione, in ceteris animalibus vitellum ex sanguine potius ita secerni, qualem veteres secretionem generatim crediderunt. His quam plurimum arridere JOANNIS MUELLER detecta in insectis, non est, cur moneam. Transitum enim vasorum in ovarii thecas exposuit. Quae cum ita sint, facile profecto perspicies, in mammalibus materia formativa per humorum congestionem advecta thecas amplificari incrassarique, atque in avibus ex eadem materia jam statim fieri secretionem. Vitello enim avium ejecto diminui quidem illam secretionem sed non prorsus sisti, inde colligo, quod, si contra regulam in avibus stigma denuo concrescit, calyx humore vitello simili impletur. Calyces hujusmodi PURKINJE pro ovis corruptis habet, at vero, nisi memoria me fallit, membrana vitelli carent.

Conclusio.

Omne animal, quod coitu maris et feminae gignitur. ex ovo evolvitur, nullum ex mero liquore formativo.

Semen virile per cuticulam ovi, nullo foramine perviam, in ovum et praeprimis in partem quandam ovi innatam agit.

Evolutio omnis e centro ad peripheriam tendit. Partes ergo centrales ante peripherieas formantur.

Idem evolutionis modus in omnibus animalibus vertebratis a rhachide incipiens.

Sed finem imponam epistolae jam aequo longiori, VESTRO judicio, quae in hac disquisitione exposui, subjiciens.

In mammalibus si quid novi inveni, id non perspectum habuisse me confiteor, nisi DOELLINGERI ac PANDERI observationibus edoctus fuissem. Quapropter voti, quod intimo pectore foveo, enunciandi veniam a VOBIS peto. Faxit Deus ter Optimus Maximus, ut VESTER PANDER pertinacissimo morbo a literis detentus pristinam recuperet valetudinem, ut cum laurea, annis jam juvenibus sibi vindicata, alias aetate virili paratas conjungat in patriae gloriam et scientiae naturalis incrementum.

Valete mihique favere nolite desinere.

Dabam Regiomonti. Prid. Iduum Quinct. 1827.

Baer.

Tabulae explicatio.

Figuras exhibitas ita disponere studui, ut earum conspectu contemplator citius ea perspiciat, quae in epistola tractavi. Hac ex causa, eaedem res, si magnitudine naturali et tum aucta magnitudine proponuntur, iisdem numeris signantur, et quidem ita, ut signa arabica magnitudinem naturalem, romana decies auctam, romana cum asterisco (II°) tricies auctam magnitudinem indicent. Ubi lineola supra numerum romanum adest, figura vicies aucta est, lineola infra numeri signum romanum quinquies auctam figuram indicat. Praeterea magnitudinis naturalis figuras in primam seriem disposui, tricies auctas in tertiam seriem.

Monendum denique est, in binis zonis atris ea, quae pellucida sunt, obscuriora videri, contra, quae alba picta sunt, albedinem non pellucidam habere. Zonas illas elegi, ut obscuritas ovulorum minorum melius in oculos incidat.

Haec jam est figurarum singularum explicatio:

in prima serie obviarum	in secunda serie obviarum	in tertia serie obviarum.
Fig. 1. Ovulum minus ex ovario canis cum disco proligero. Magn. nat. *Obs.* Symmetriae causa tantum figurae auctae hunc in magnitudine naturali adjicere volui, licet ovum tam parvum sit, ut a sculptore ita exarari non possit, nam ab oculis acutioribus vix percipitur.	*Fig. I.* Idem decies auctum.	*Fig. I.°* Idem tricies auctum ad discum proligerum plenius demonstrandum.
Fig. 2. Ovulum canis ex ovario, maturius, cum disco proligero. Magn. nat.	*Fig. II.* Idem decies auctum. Pars disci proligeri ablata est, ut membrana corticalis jam formata in conspectum veniat.	*Fig. II.°* Ejusdem globulus dilaceratus ad demonstrandam cavitatem.

Fig. 3 Ovum canis ex tuba Fallopii. Magn. nat.

Fig. 4. Ovum canis ex utero, jam pellucidum, diametro $\frac{1}{4}$ lin. Magn. nat.

Fig. 5. Ovum canis ex utero magis evolutum et pellucidum, diametr. $\frac{1}{4}$ lin. Magn. nat.

Fig. 6. Ovum caninum duodecim dierum cum embryone magnitudine naturali (ad iconem, quam dederunt Prévost et Dumas, recusum.)

Fig. 7. Ovum caninum trium circiter hebdomadum, magnitudine naturali.

Fig. III. Idem decies auctum.

Fig. IV. Idem decies auctum et quidem in.

Fig. IV.[a] ita visum, ut blastodermatis primordium infimum locum teneat, quare obscurius apparet;

Fig. IV.[b] a latere visum post membranarum separationem.

Fig. V. Idem decies auctum.

Fig. V.[a] post multas horas valde collapsum, blastodermate a latere viso.

Fig. V.[b] Turgens, blastodermate locum supremum tenente.

Fig. VI. Ejusdem ovi fetus auctus.

Fig. VII. Ejusdem ovi fetus decies auctus, a latere et sectione transversa pictus. Apparet enim in

Fig. III.[a] Idem tricies auctum.

Fig. IV.[a] Ejusdem ovi particula membranae internae tricies aucta.

Fig. V.[a] Ejusdem ovi particula membranae internae tricies aucta.

Fig. VII.[a] Villi ejusdem ovi tricies aucti.

Fig. VII.[a] Fetus caninus, lateri sinistro incumbens, resecta vaginae capitis parte dextra, nec non sacci intestinalis parte dextra.

a. b. Transitus laminae pituitosae sacci intestinalis in vaginam capitis.
a. c. d. e. Vagina capitis dissecta.
e. f. Vena descendens dextra reclinata. f" locus, quem primitus tenuerat.
e. g. Atrium cordis.
h. Ventriculus cordis.
i. i. Pericardium.
k. Bulbus.
l. Aorta.
m. n. o. Encephalon.
m. Auris et medulla oblongata.
n. Corpus quadrigeminum.
o. Oculus et cerebrum.
p. Aditus ad intestinum crassum.
e. p. Sutura vel sulcus intestini medii.
q. r. e. p. Intestini paries sinister.
s. t. e. p. Intestini paries dexter.
q. r. Angulus, quo paries intestini sinister in saccum intestinalem transit.
u. v. w. Area vasculosa.
q. r. w. Area pellucida.
u. s. Vena ascendens.
x. y. * et * * Amnion.
z. Saccus urinarius s. Allantois.

Fig. VII.[b] Ejusdem embryonis sectio transversa in medio dorso facta, decies aucta.
a. Dorsi culmen.
a. b. Lamina dorsalis.
b. c. Lamina ventralis.
d. Saccus intestinalis.
e. Angulus, quo intestini paries in sacum intestinalem transit.
d. e. ergo limbus secundus Wolffii.
f. Sutura intestini apud Wolff.
g. Columnae vertebralis primordium.
h. Chorda dorsalis.
i. Amnion.
k. Medulla spinalis.

Fig. VIII. Corpora aliena, in utero et tubis obvia, decies aucta.
I. Bullae oleosae.

II. Concrementa albuminosa.
III. Telae cellulosae particulae.
IV. Membranae granulosae frustula.

Fig. IX. Vesicula Graafiana (mediae magnitudinis) scrofae, decies aucta, ad axin dissecta.
1. Epithelium peritoneale.
2. Tela formativa (*stroma*).
3. Stratum externum } thecae.
4. Stratum internum } thecae.
xx Stigma
5. Membrana granulosa } nuclei.
6. Fluidum contentum } nuclei.
7. Discus proligerus } nuclei.
8. Ovulum } nuclei.

Fig. X. Discus proligerus suis cum fluidi condensati parte.
Fig. XI. Membranae granulosae frustulum, decies auct.
Fig. XII. Ovulum cum disco proligero vaccae, decies auct.
Fig. XIII. Ovulum cum disco proligero et membrana granulosa hominis, decies auctum.
Fig. XIV. Corpus luteum canis, decies auct.
1. Epithelium peritoneale.
2. Tela formativa.
3. Thecae stratum externum.
4. Corpus luteum.
5. Ejus apertura.
6. Massa albuminosa.
7. Corporis lutei cicatricula inchoata.

Fig. XV. Stigma vesiculae Graafianae suis deorsum inspectum, decies auctum.
Fig. XVI. — XXX. Animalium inferiorum ova, vel horum partes.
Fig. XVI. — XVIII. Ova serpentum.
Fig. XVI. Stratum proligerum ex ovo Colub. Natricis maturo, quinquies auctum.
Fig. XVII. Ejusdem vesicula Purkinji immatura, decies aucta.
Fig. XVIII. Eadem matura, decies auct.
Fig. XIX — XXII. Ova lacertarum.
Fig. XIX. Stratum granulosum ex ovo Lacertae agilis, dec. aut.
Fig. XX. Lacertae agilis ovum junius, decies auct.

Fig. XXI. Vesicula ex ovo fere maturo Lacertae agilis, dec. auct.

Fig. XXII. Vesicula ex ovo maturo Lacertae croceae, dec. auct.

Fig. XXIII. — XXVI. Ova ranarum.

Fig. XXIII. Ovum immaturum Ranae temporariae, dec. auct. † Locus ubi stratum proligerum oritur.

Fig. XXIV. Ovum Ranae temp. fere maturum, acido nitrico diluto condensatum ···et ad axin longiorem dissectum, decies auctum.

NB. Colores tales dedimus icuni, quales ovum raninum in acido nitrico diluto induit, quis bis optime partes diversae discernuntur.

Fig. XXV. Idem ovum maturum propullante vesicula, dec. auct.

Fig. XXVI. Idem ovum expulsa vesicula, dec. auct.

In tribus ultimis figuris notavimus cum signo

a, membranam vitelli,

b, blastoderma in statu evoluto (Fig. XXVI) et in statu immaturo, quo stratum proligerum nostrum est,

c, vitellum,

d, vesiculam Purkinji.

Fig. XXVI. Ejusdem ovi pars suprema vicies aucta.

Fig. XXVII. — XXX. Astaci fluviatilis ova.

Fig. XXVII. Ovum junius Astaci fluviatilis cum disco proligero deorsum inspectum, decies auctum.

Fig. XXVIII. Idem a latere.

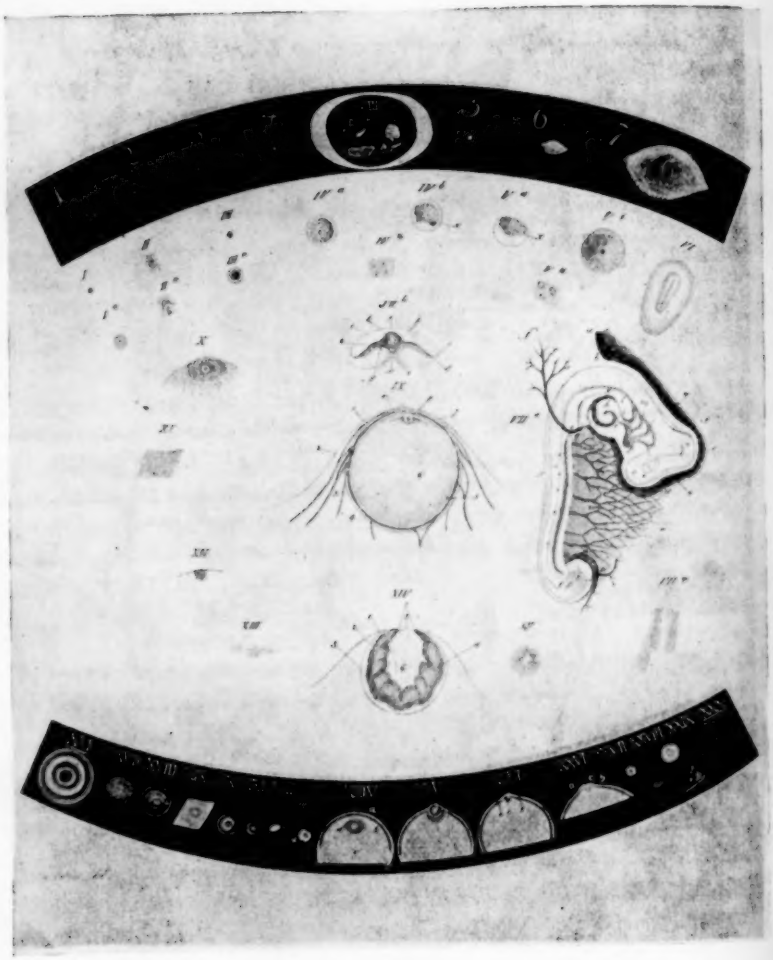
Fig. XXIX. Ovum provectius.

Fig. XXX. Ovum maturum cum blastodermatis primordiis, quinquies auctum.

Corrigenda.

Pag.	9	vers.	3	pro consecta	lege: confecta
—	10	—	6	— Fig. V. —	Fig. V°.
—	12	—	2	— «pai —	«pot
—	14	—	10	— delphinum, phocaenam	lege: delphinum phocaenam
—	14	—	23	— vesiculis	lege: vesiculae
—	17	—	ultimo	vox: a	deleatur
—	23	—	1	— catenae	lege: centenae
—	31	—	1	— apparet —	appareat
—	31	—	23	— similibus —	similia

Priorem libelli partem perlegens, quam ex typotheca jam remissam habeo, vereor ne ea, quae pag. 4ta de canalis intestinalis origine obiter docui, lectores in errorem ducant. Clarius ergo ita exponam. intestini fundus non ex coalitione duarum plicarum oritur. Conflescunt quidem plicae strati vasculosi blastodermatis, quo fit mesenterium; stratum mucosum vero inde protruditur et fundum intestini cum parietibus constituit. Quae alio loco fusius demonstrabo.



The First Sighting of the Antarctic Continent

A CRITICAL ANALYSIS OF BISCOE'S DISCOVERY OF ENDERBY LAND

One hundred years ago from February twenty-eighth last, members of the crews of two small sealing vessels sent out by the famous firm of ENDERBY Bros. of London sighted what they took to be land near the Antarctic circle in that sector of Antarctic south of Africa (1). The two ships, the *Tula*, 148 tons, and the *Lively*, 50 tons, were under the command of JOHN BISCOE, retired Master R. N. and veteran in the employ of ENDERBY Bros. All lingering doubt as to the actual existence of land in the region in which this expedition reported it to exist has been recently removed by the verification of BISCOE's reports by two expeditions which operated in the same area almost simultaneously some ninety-nine years subsequent to the date of BISCOE's sighting of the land to which he gave the name « Enderby's Land » (2). Not only did the two recent expeditions, the *Norvegia*

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(1) MURRAY, GEO., (edit.), *The Antarctic Manual*, Royal Geographical Society, London, 1901, pp. 305-335, JOHN BISCOE, « Journal of a Voyage toward the South Pole on board the brig « Tula », the cutter « Lively » in company, specific reference on cruise under discussion, pp. 305-328; *Journal of the Royal Geographical Society*, vol. 3, 1833, pp. 105-112, (CHARLES ENDERBY), « Recent discoveries in the Antarctic Ocean. From the log book of the Brig « Tula », commanded by Mr. JOHN BISCOE, R. N. Communicated by Messrs. ENDERBY. Read 11th February, 1833; » *Nautical Magazine*, vol. IV, 1835, pp. 265-275, « Voyage of the « Tula » towards the South Pole. » The bibliography in *The Antarctic Manual* also lists an earlier publication of BISCOE's journal : BISCOE, JOHN, *Journal of a Voyage towards the South Pole, 1830 to 1832*, Edinburgh, 1834 : see *The Antarctic Manual*, p. 533.

(2) This seems to have been the original form (*Journal of the Royal Geographical Society*, vol. 3, *loc. cit.*, p. 109).

Antarctic Expedition of 1929-1930 (3) and the British, Australian, New Zealand Antarctic Research Expedition of 1929-1930, (4) verify BISCOE's reports of land; but by their extending of the land mass that he sighted in both directions and thus narrowing the gap of unknown territory separating it from other known Antarctic land areas, they also distinctly added to the probability that the sector which he sighted is a portion of an Antarctic continental land mass. If this supposition should be correct, then since BISCOE's sighting of Enderby Land is the earliest recorded sighting of an Antarctic land area which is now thought to form an integral part of the still somewhat hypothetical continent, it is to some extent justifiable to refer to his sighting as « the first sighting of the Antarctic continent » (5).

More than eight months before making the important discovery just mentioned, BISCOE's two ships had set sail from England. On November 27, 1830 they had left Berkeley Sound, Falkland Islands bound south on the first stage of what is now recognized as one of the greatest feats of high southern navigation ever accomplished. BISCOE's first objective was the South Sandwich Islands, which were successfully sighted on December 21. An attempt to press southeast from here to investigate an « appearance of land » proved perilous and ended in a retreat back to the Sandwich Islands, which were reached for the second time on December 29(6).

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(3) *Geographical Review*, vol. 20, 1930, pp. 555-573, HJALMAR RIISER-LARSEN, « The ' Norvegia ' Antarctic Expedition of 1929-1930; » JOERG, W. L. G., *A Brief History of Polar Exploration Since the Introduction of Flying*, 2nd revised edition, American Geographical Society, New York, 1930, pp. 21-25.

(4) *Geographical Review*, vol. 20, 1930, pp. 535-554, Sir DOUGLAS MAWSON, « The Antarctic Cruise of the ' Discovery ', 1929-1930; » JOERG, *op. cit.*, pp. 28-34.

(5) After the recent lesson in connection with the « Graham Land Peninsula », which Sir HUBERT WILKINS showed to be an archipelago, it is doubtful whether many geographers would care to advance the positive unreserved opinion that Enderby Land is part of the Antarctic Continent until the gaps now separating it from other Antarctic land areas thought to form the continent have been filled in. On « Graham Land Peninsula » see especially *Geographical Review*, vol. 19, 1929, pp. 353-376, Sir HUBERT WILKINS, « The Wilkins-Hearst Antarctic Expedition, 1928-1929; » *ibid.*, pp. 377-386, Dr. H. R. MILL, « The Significance of Sir HUBERT WILKINS' Flights; » JOERG, *op. cit.*, pp. 3-11 : on BISCOE as discoverer of the Antarctic Continent compare TAYLOR, GRIFFITH, *Antarctic Adventure and Research*, D. APPLETON & Co., New York and London, 1930, specific reference, pp. 20-22.

(6) In respect to BISCOE's operations in the vicinity of the South Sandwich

From these islands the course was now set approximately eastward. January 17, 1831, BISCOE's ships crossed the parallel of sixty south almost at the same point BELLINGSHAUSEN crossed it eleven years earlier; five days later they crossed the Antarctic circle in 1° E. long. For the next five weeks BISCOE was able to sail either on or south of the Antarctic circle, by the end of that period having reached about 50° E. long. He reached his farthest south of the first half of his circumnavigation on January 28, attaining 69° S. lat. in 10° 43′ E. long. The course followed adhered as closely as possible to the northern edge of the field-ice. Brisk easterly winds combined with the proximity of the ice to make the passage a strenuous one.

During the first week of February 1831, BISCOE managed to maintain his course south of the Antarctic circle, most often between 68° and 69° S. lat. On February 1st and again on February 4th « appearances of land » were noted; the position on the 4th was 68° 30′ S. lat. 14° 42′ E. long. Indications of the proximity of land were again observed on February 7th, but no soundings were obtained with 250 fathoms of line. Despite heavy winds, the *Tula* and her consort continued to keep to an easterly course throughout the following fortnight. Captain COOK's track of 1773 was crossed on the 19th, and the field-ice was found to be occupying practically the same position that it had at the time of the famous earlier navigator's visit.

« Appearances of land » were again sighted on February 25th. Later the same day, these appearances resolved themselves into a vast structure of ice stretching away as far as the eye could see, but showing no sign of land. The cliff face of the phenomenon had much the aspect of North Foreland. In keeping with his preconceived hypothesis of the sea origin of ice, BISCOE arrived at the conclusion that even this great mass was not necessarily attached to land but had probably been formed afloat at sea. However, he was also of the opinion that it would be impossible for a ship to push any farther south in that area.

Islands, it may be noticed that he seems to have lacked knowledge of the work accomplished in this same area by BELLINGSHAUSEN, though Traversy (or Travers, as BISCOE gives it) Island was marked on his charts : see *The Antarctic Manual*. *loc. cit.*, p. 307.

Three days later, February 28, 1831, (7) land was distinctly seen. To quote from BISCOE's journal : « Feb. 28.—In the morning more regular sea. Tacked to the southward. Wind S. E. Noon more clear. Lattitude, 65° 57′ S., longitude, 47° 20′ 30″ E. P. M., passed to the southward through much broken field-ice. 4 p. m., saw several hummocks to the southward, which much resembled tops of mountains, and at 6 p.m. clearly distinguished it to be land and to considerable extent; to my great satisfaction what we had first seen being the black tops of mountains showing themselves through the snow on the lower land, which, however, appeared to be a great distance off, and completely beset with close field-ice and icebergs. The body of the land bearing S. E. » (8)

At the time of his original sighting, BISCOE had little time for investigation; for shortly severe gales sprang up which forced his ships northward during the next day and a half. Not until late in the afternoon of March 2nd, was he again able to make his way southward to within sight of the newly discovered land. A bold headland which appeared to be a cape and which was later named « Cape Ann » (9) was made out. The main body of the land seemed to stretch from southeast to southwest about thirty-six miles south from 66° 07′ S. lat., 49° 06′ E. long. Cruising eastward during that night, which was marked by the most beautiful appearance of the Aurora Australis noted during the voyage and which so attracted the crews' attention that it was difficult to keep the men active in sailing the vessels, the two ships the next morning rounded what seemed to be the eastern extremity of the ice and were able to penetrate for fifteen miles due southward into a channel between a chain of immense ice-islands

(7) There seems to be some disagreement as to the date of this sighting; the present writer here follows Dr. H. R. MILL (MILL, HUGH ROBERT, *The Siege of the South Pole*, STOKES & Co., New York, 1905, p. 155) and his interpretation of the entry in BISCOE's journal (*The Antarctic Manual*, *loc. cit.*, p. 323) : British Admiralty Chart No. 3170, « Antarctic Regions, Sheet 1, Between Latitudes 60° S. and 75° S. and Longitudes 2° E. and 55° E. » New Edition 31st July 1914; *Journal of the Royal Geographical Society*, vol. 3, *loc. cit.*, p. 108; BALCH, E. S., *Antarctica*, Philadelphia, 1902, pp. 118-119 give February 27th as the date.

(8) *The Antarctic Manual*, loc. cit., pp. 322-323.

(9) The form « Cape Ann » is almost universally found; British Admiralty Chart No. 3170, however, uses « Cape Anne ».

and firm field-ice. In the end, however, just as the way through to the land seemed clear, the ships were forced to return northward just escaping being beset in a closing of the channel. After again reaching open water, BISCOE steered along the edge of the field-ice, at noon of March 4th finding himself at 65° 42′ S. lat., 49° 29′ E. long. Bearings taken at 4 p. m. of the same day placed Cape Ann in lattitude, 66° 25′ S., longitude, 49° 18′ E.

That night a breeze sprang up, which by the afternoon of the next day was a stiff gale. By the mid-night of the 5th, the storm had become a veritable hurricane. It lasted without intermission until the morning of the 8th. So violent was the gale that it not only drove the *Tula* 120 miles to the N. N. W., but caused considerable damage to the vessel. The cutter *Lively* was lost track of early during the storm.

Despite these handicaps, BISCOE was determined to make another effort to approach more closely the land. There are few stranger incidents in the history of polar exploration than that of this veteran sea captain who had so convinced himself of the small possibility of the existence of land in the area that when he saw what his sense of sight told him must be land he must be so doubtful as to feel it necessary to strive futilely to reach it in order to make sure, despite overwhelming odds against him. Although one of his ships was missing, the other damaged and a good portion of her crew incapacitated, yet BISCOE battled for nearly two weeks more in an attempt to get closer to the land. This final effort met with some degree of success. On March 16, his ship again came within sight of land. First a high mountain and later Cape Ann were seen. But ice still prevented a close approach to the land and BISCOE was forced to at last acknowledge the impracticability of his endeavor and change his course to the northward. At noon of the sixteenth, he was in 65° 16′ S. lat., 49° 27′ E. long.

However, now that BISCOE had abandoned his attempt to fight his way through to the land, adverse winds made his progress away from the area slow and difficult. From March 16 to March 22, he was held almost in the same position, and even by March 24 had reached no farther than 62° 26′ S. lat., 52° 24′ E. long. BISCOE still continued exploratory work, for some weeks holding as close as possible to the northern edge of the field-ice and in general

keeping within 60° south. In the vicinity of 80° E. he was forced to abandon even this endeavor and to head farther north setting a course for New Zealand. Because of the shorthandedness of the crew due to sickness, this objective had to be surrendered. In the end after a tremendous battle with the elements, the *Tula* managed to reach what is now Hobart, Tasmania on May 10, 1831. The cutter *Lively*, though not lost, fared even worse. After her separation from the *Tula*, she had retreated northward setting a course for Australia. Her crew had become so greatly depleted that it was only by the utmost chance that she succeeded in reaching Port Philip, Australia, where the crew landed to recuperate and secure provisions. During a storm, the cutter drifted away, but was re-located a fortnight later having been stranded ashore. She was re-fitted and finally put to sea, reaching the Derwent on September 3rd, just as BISCOE was putting out to sea on the second cruise of his circumnavigation.

Before entering upon a critical comparison of BISCOE's reports with those of the more recent expeditions to operate in the same area, brief mention of the concepts concerning what BISCOE had discovered which were held by the explorer himself and his contemporaries may perhaps be made. As for the view of BISCOE himself, he seems to have been of the belief that the lands which he had here sighted were derived from perpendicular rocks and not very great in extent. Approximately a year later, when commenting upon his discovery of Adelaide Island in the American Antarctic Archipelago, he remarked in his journal, « From the great depth of the water I consider this island (Adelaide I.) to have been originally a cluster of perpindicular rocks, and I am of opinion that the land that I before saw last year, could I have got to it, would have proved to be in the same state as this, and likewise all land found in high southern latitudes. » (10) On the other hand, geographers in England (11) and France (12) seem to have considered his discovery of Enderby Land in conjuction with that of Graham Land as the possible sighting of the outposts

(10) *The Antarctic Manual*, *loc. cit.*, p. 331.

(11) *Journal of the Royal Geographical Society*, vol. 3, *loc. cit.*, « Note », p. 112 (presumably by JOHN BARROW).

(12) *Bulletin Société Géographique, Paris*, vol. 19, 1833, pp. 165-167, « Nouveau Continent découvert par le Capt. Anglais BISCOE commandant le brick ' Tula '. »

of a south polar continent yet to be placed upon the maps. A geography published in America, to the contrary, definetly classified Enderby Land as an island. (13)

During the hundred years which have passed since BISCOE's operations, three expeditions besides the two already mentioned approximated the position in which BISCOE reported land. But two years subsequent to BISCOE's explorations, another sealer in the employ of ENDERBY Bros., KEMP by name, reported having discovered land in about the same latitude some ten degrees east of BISCOE's position. (14) MOORE, in the *Pagoda* (1845), reached within seventy miles of the recorded position of Enderby Land without sighting any signs of land. (15) While the German Deep Sea Expedition in the *Valdivia* (1898-1899) which approached within close to a hundred nautical miles of the given position failed to sight any actual indication of land, dredgings made by the expedition evidenced the possible existence of land of continental type not far distant. (16) Attention might also be drawn to the fact that prior to BISCOE's operations, the American sealer MORELL seemingly penetrated the same general area without sighting any signs of land. In fact, if certain interpretations of MORRELL's meager narrative are followed, he seems to claim to have sailed south of the position later assigned to Enderby Land by BISCOE. (17)

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(13) MURRAY, HUGH, *The Encyclopedia of Geography*, Philadelphia; CAREY, LEA & BLANCHARD; 1837 : specific reference, Vol. III, pp. 172-173. Cited by BALCH, *op. cit.*, p. 123n.

(14) MILL, *Siege of South Pole*, p. 167. KEMP's report was confirmed by the B., A., N. Z. Antarctic Research Expedition : *Geographical Review*, vol. 20, MAWSON, *op. cit.*, specific reference, pp. 546-547, 552.

(15) *Nautical Magazine*, London, 1846, pp. 21-22, (Lieut. T. E. L. MOORE), « Magnetic Voyage of the 'Pagoda'; » *United Service Magazine*, London, 1850, Pt. II, pp. 201-208, W. D. (WALTER DICKSON), « The Antarctic Voyage of Her Majesty's Hired Barque 'Pagoda'. »

(16) CHUN, CARL : *Aus den Tiefen des Weltmeeres* : Jena, GUSTAV FISCHER, 1900; *Zeitschrift der Gesellschaft Erdkunde zu Berlin*, Band XXXIV, Jahrgang 1899, Berlin, 1899, pp. 75-192, « Die Deutsche Tiefsee-Expedition. »

(17) MORRELL, BENJAMIN : *A Narrative of four voyages to the South Sea, North and South Pacific Ocean, Chinese Sea, Ethiopic and Southern Atlantic Ocean, Indian and Antarctic Ocean. From the year* 1822-1831 ; New York, 1832; specific reference, pp. 59-69. Divergent critical analyses of MORRELL's narrative may be found in MILL, *Siege of the South Pole*, pp. 105-109 and BALCH, *op. cit.*, pp. 100-101, 102-104.

To turn, now, to an analytical comparison of BISCOE's reports and the findings of the expeditions which most recently operated in the same area, it cannot but be repeated that the results of these expeditions have definitely removed any doubt of the veracity of BISCOE's reports that might exist. It can now be taken as established that land exists in the general position in which BISCOE placed it. However, from the correction made by the two latest expeditions of the longitudes of such specific places as could be identified, it would seem that BISCOE's longitudinal positions should be rectified by being changed one degree to the east (18). BISCOE reported a sector of coast extending from about 46° E. to 56° E. long. (19) Findings of the two most recent expeditions indicate the existence of a 250 mile wide land mass that projects out from the hypothetical main body of the Antarctic continent between 50° E. and 59° E. long. (20)

In regard to the suggested correction of BISCOE's longitudinal positions, it can be remarked that if the correction is made, identification of BISCOE's specific landfalls possesses little difficulty; while if it is not applied it is practically impossible to make his reports fit the area as it now has been charted. Applying the correction, BISCOE's original landfall of tops of low mountains showing above the lower land (21) is identifiable as the Scott Mts. of the reports of the British, Australian, New Zealand Antarctic Research Expedition under MAWSON (22). BISCOE records that he sighted the tops of mountains a long distance off to the southeast at 4 p. m. of February 28, for which his corrected noon position would be 65° 57′ S. lat., 48° 20′ 30″ E. long. (23); while MAWSON's expedition placed the Scott Mts. in about 67° S. lat., 49° 30′ to

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(18) It may be noted that this correction had already been applied in British Admiralty Charts of the area even previous to the operations of the two recent expeditions in the locality. Vide : British Admiralty Chart No. 3170 New Editions 31st July 1914, and 3rd June 1927.

(19) *Journal of the Royal Geographical Society*, vol. 3, 1833, facing p. 96, « Part of the Southern Hemisphere Shewing Recent Discoveries; » British Admiralty Chart 3170 : cf. TAYLOR, *op. cit.*, p. 22.

(20) *Geographical Review*, vol. 20, p. 552, Editorial Note (W. L. G. JOERG); JOERG, *op. cit.*, p. 32 and map p. 24.

(21) *The Antarctic Manual*, pp. 322-323.

(22) *Geographical Review*, vol. 20, MAWSON, *op. cit.*, p. 550.

(23) *The Antarctic Manual, loc. cit.*

51° E. long. (24) With the rectification made, BISCOE's Cape Ann (25) is identifiable as the feature to which the *Norvegia* Expedition under RIISER-LARSEN attached the name Cape Ann (26) and to which MAWSON's Expedition in the *Discovery* affixed the name « Mt. Biscoe » (27). Comparison of photographs taken by the respective recent expeditions show that the divergent names apply to the same feature (28); and the corrected position assigned by MAWSON, 66° 25′ S. lat., 50° 18′ E., long. (29) fits well with the approximate position of RIISER-LARSEN (30). Also, it may be said that MAWSON's position coincides very closely with BISCOE's as corrected (31) and that BISCOE's fragmentary description of his « Cape Ann » (32) agrees in general with that of the feature as depicted in the photographs of both the recent expeditions. Another indication of the advisability of the shift is to be found in the identification of BISCOE's « Mt. Codrington » by MAWSON's expedition (33).

While the recent operations in the area have thus in general endorsed the ninety-nine year earlier reports of BISCOE, there is one respect in which they show a marked change from the conditions as reported by the earlier explorer. This aspect is the disappearance of the shelf-ice, which almost surely must be the true nature of the ice phenomenon that BISCOE sighted on February 25, 1831 (34). RIISER-LARSEN of the *Norvegia*, which

(24) *Geographical Review*, vol. 20, MAWSON, in *loc. cit.* See also « British, Australian, and New Zealand Antarctic Research Expedition, Sir DOUGLAS MAWSON in command », *Geographical Review*, vol. 20, No. 4, Pl. VI, facing p. 552, « Map Showing the Discoveries of the Expedition along the Coast of the Antarctic Continent between 49° and 81° E. »

(25) *The Antarctic Manual*, p. 323.

(26) *Geographical Review*, vol. 20, RIISER-LARSEN, *op. cit.*, pp. 562-563.

(27) *Idem.*, MAWSON, p. 550.

(28) Cf. *Geographical Review*, vol. 20, p. 549, Fig. 12 and *idem.*, p. 563, Fig. 10. See also JOERG, *op. cit.*, pp. 23, 32-33, 32n.

(29) *Ibid.*, p. 32.

(30) *Ibid.*

(31) BISCOE, as noted above, assigned 66°25′ S. lat., 49°18′ E. long. (*The Antarctic Manual*, p. 323). With the suggested addition this would obviously be almost identical with the position assigned by MAWSON.

(32) *The Antarctic Manual*, p. 323.

(33) *Geographical Review*, vol. 20, MAWSON, p. 547. MAWSON must have drawn upon unpublished manuscript charts; the place name does not appear on any of the published maps examined by the present writer.

(34) *The Antarctic Manual*, pp. 319-320.

on February 15-16, 1930, nearly duplicated the position of the *Tula*, specifically speaks of the disappearance of the barrier (35). The narrative of MAWSON's expedition, which passed near the location on January 23, mentions only the existence of an ice tongue bounded by cliffs in 66° 22′ S. lat., 48° 31′ E. long. (36) Seemingly the great expanse of ice which BISCOE mentions as stretching away for thirty or forty miles from 66° 02′ S. lat., 44° 54′ E. long. (corrected) (37) has been broken up and carried away to sea during the ninety-nine year interval perhaps leaving the ice-tongue sighted by MAWSON as a remnant of its once greater extent.

In addition to this feature and the definite reports of land which have already been discussed, certain indications of land reported by BISCOE in various other positions are worthy of attention in the light of the results of recent expeditions. Especially is this true of the series of indications of land reported by him in varied positions in 67° to 69° S. lat., from 24° to 28° E. long. (corrected) (38). These indications located in an area in which the *Norvegia* was forced northward by the unusual congestion of ice, (39) now seem quite likely to have some relationship to land not far to the south. The probable trend of the coast as indicated by the discoveries of RIISER-LARSEN's expedition would tend to indicate the existence of land within a hundred and fifty miles of the positions mentioned by BISCOE (40). While no land seems to exist near the earlier navigator's recorded position of an « appearance of land » to the east of Enderby Land within sight from about 60° 43′ S. lat., 56° 17′ E. long. (41), it cannot

(35) *Geographical Review*, vol. 20, RIISER-LARSEN, p. 565.

(36) *Idem*, MAWSON, p. 551.

(37) *The Antarctic Manual*, *ibid.*

(38) *Idem*, pp. 316-318.

(39) *Geographical Review*, vol. 20, RIISER-LARSEN, p. 567.

(40) *Idem*, facing p. 572, ' Norvegia ' Antarctic Expedition of 1929-1930, Commander H. RIISER-LARSEN, in command : » (*Geog. Rev.*, vol. 20, No. 4, Pl. VII) : « Map Showing the Field of Operations of the Expedition. » According to recent news dispatches published subsequently to the writing of the article, the ' Norvegia ' Expedition in the course of operations undertaken during the ensuing season (1930-1931) and involving a circumnavigation of Antarctica actually discovered land extending from 70° 30′ S., 24° 15′ E. to 68° 45′ S., 33° 30′ E., which was named Princess Ragnhild Land.

(41) *The Antarctic Manual*, p. 325.

but be pointed out that BISCOE's general hypothesis of the existence of further land to the east of his Enderby Land (42) was shown to be correct by MAWSON's expedition. The reports of the *Discovery* expedition have established that land actually exists in the region though some five degrees south of the position of BISCOE's « appearance of land » (43).

To conclude, looking back one cannot but pay tribute to this veteran sealing captain and his men. His valiant battle to reach the land against what such Antarctic veterans as Sir DOUGLAS MAWSON and Captain J. K. DAVIS describe as the worst summer gales they had ever experienced cannot but stir one's admiration. Neither the *Norvegia* not the *Discovery* with the aid of steam approached much more closely to the coast than BISCOE forced his smaller sailing ships. Considering the well known difficulties of making accurate observation in the period that BISCOE made his voyage, one cannot but compliment him on the general accuracy of his observations. An error of but one degree is none too common, especially in Antarctic reports of that time. That land exists in the general region in which BISCOE reported it, has been definitely established beyond any shadow of a doubt. Moreover the limits of the specific Antarctic land area of which BISCOE was the first to sight a portion have already been greatly extended in both directions, and indications which point to the probability of its being joined to Wilhelm Land on the east and the newly discovered Crown Princess Martha Land on the west by a continuous coast yet to be filled in and thus forming an integral part of an Antarctic continent have been noted. Should this latter supposition be correct, then BISCOE is entitled to the honor of being the first man to sight the Antarctic continent, the only continent which Caucasians can lay a just claim to having truly discovered.

APPENDIX :
POSSIBLE RELATIONSHIP OF HORSBURGH'S HYPOTHESIS TO BISCOE'S VOYAGE

In his discussion of BISCOE's voyage in *The Siege of the South Pole*, Dr. HUGH ROBERT MILL advances the contention that the

(42) *Idem.* p. 326.
(43) *Geographical Review*, vol. 20, MAWSON, pp. 546-547.

hypothesis put forward by Captain JAMES HORSBURGH, sometime hydrographer to the East India Co., that land existed in the far south influenced to some degree the voyage of BISCOE (44). The hypothesis refered to by Dr. MILL was advanced by HORSBURGH in a paper read before the Royal Society of London on February 4, 1830 (45). Basing his supposition upon the numerous icebergs sighted in the South Seas, HORSBURGH contended that « the existence of a large tract of land near the Antarctic circle seems to be necessary for its origins » (46). He held that this land probably lay between the meridian of Greenwich and 20° E. long. (47)

While a careful reading of BISCOE's journal as published in *The Antarctic Manual* (48) fails to reveal any evidence of knowledge on BISCOE's part of HORSBURGH's prediction, it should not be taken that Dr. MILL's implication is thereby disproved. It should be remembered that BISCOE was an employe of a commercial firm and as such operated under instructions. Mr. CHARLES ENDERBY, one of the two brothers heading the famous firm of ENDERBY Bros. of London, BISCOE's employers, is known to have been one of the original fellows of the Royal Geographical Society. He is known to have had a marked interest in the advancement of science, was quite likely aware of HORSBURGH's paper, and may have been influenced by it in drawing up the firm's instructions to BISCOE. As far as BISCOE is concerned, however, attention cannot but be drawn to the marked divergence between his view on the origin of sea ice and icebergs and that of HORSBURGH. BISCOE's insistence on the sea origin of all Antarctic ice forms (49) offers a marked contrast to HORSBURGH's basing a supposition of the existence of an undiscovered land area upon the observation of drifting Antarctic ice.

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(44) MILL, *Siege of the South Pole*, p. 148.

(45) *Philosophical Transactions of the Royal Society of London for the year MDCCCXXX*, pt. 1, (vol. 120, pt. 1, pp. 117-120), « No. VII—Remarks on several icebergs which have been met with in unusually low latitudes in the Southern Hemisphere : By Captain JAMES HORSBURGH, Hydrographer to the East India Co. Read Feb. 4, 1830. »

(46) *Idem*, specific reference, pp. 119-120.

(47) *Ibid.*

(48) See Note No. 1, *supra*.

(49) *The Antarctic Manual, loc. cit.*, specific reference, pp. 317, 319-322.

In passing it may be noted that HORSBURGH was not the first writer to advance the ice found drifting in the sub-Antarctic as evidence of the existence of an extensive land area in the far south (50). Also another hypothesis advanced by HORSBURGH in the same paper and which is perhaps worthy of mention though lacking of specific connection with the present discussion is his advocacy of earthquakes as the prime cause of the unusaul outbursts of ice from the Antarctic which result in icebergs being found in unusually low southern latitudes (51). As recently as twenty years ago this hypothesis found favor with no less an authority than Dr. MILL (52). At the present time, however, it seems to be being displaced by a more recent theory advanced especially by OTTO PETTERSSON, well known hydrographer, which sees these unusual outbursts as being produced by submarine tidal forces caused by the position of the earth relative to the sun and moon coinciding with the perhelion apside of the moon (53).

(Los Angeles, California.) EDWARD W. DALTON.

LIST OF MAPS CONSULTED

1. *Journal of the Royal Geographical Society*, vol. 3, 1833, facing p. 96, « Part of the Southern Hemisphere Shewing Recent Discoveries. »

2. *The Antarctic Manual*, London, 1901, pocket at rear, « Antarctic Ocean Sheet No. 3 : Between Latitudes 40° S. and 80° S. ; Longitudes 70° W. to 90° E. »

3. MILL, H. R., *The Siege of the South Pole*, New York, 1905, at end of book, « Chart of South Polar Regions by J. G. BARTHOLOMEW : Compiled from the Latest Sources : 1905. »

4. British Admiralty Chart No. 3170 : « Antarctic Regions, Sheet I, Between Latitudes 60° S. and 75° S. and Longitudes 2° E. and 55° E. » New Edition 31st July 1914, and New Edition 3rd June 1927.

5. *Geographical Review*, vol. 20, 1930, No. 4, Pl. VI, « British, Australian, and New Zealand Antarctic Research Expedition, Sir DOUGLAS MAWSON, in command. »

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(50) VIDEO BALCH, *Antarctica*, p. 61.

(51) *Philosophical Transactions of the Royal Society of London*, vol. 120, *loc. cit.*, pp. 119-120.

(52) *Encyclopaedia Britannica*, 11th Ed., vol. 21, « Polar Regions », specific reference p. 970.

(53) *Geographical Review*, vol. 19, 1929, pp. 121-131, OTTO PETTERSSON, » Changes in the Oceanic Circulation and their Climatic Consequences. »

6. *Geographical Review*, vol. 20, 1930, No. 4, Pl. VII, « ' Norvegia ' Antarctic Expedition, Commander H. RIISER-LARSEN, in command. »

7. JOERG, W. L. G., *A Brief History of Polar Exploration since the Introduction of Flying*, New York, American Geographical Society, 1930, p. 24, « Map of Enderby Land and the newly discovered lands in its vicinity as mapped in 1929-1930 by MAWSON and RIISER-LARSEN, together with the tracks of the *Discovery* and the *Norvegia*. (From original manuscript maps furnished the American Geographical Society by both leaders.) »

The Origin of the Ghubār Numerals, or The Arabian Abacus and the Articuli

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I. — INTRODUCTION

1. *The Hindu and the Ghubār Numerals.* — The modern numerals with place value and zero are commonly known as the *Arabic* numerals, as distinguished from the *Roman* numerals. The names *Arabic* and *Roman* simply indicate the origin and source, they acknowledge the fact that the first numerals were received from the Arabs and the latter ones from the Romans. The Arabs, too, distinguished two different types of numerals and characterized them by two names, the *Hindu* and the *ghubār* numerals. The Hindu numerals were common among the Eastern Arabs and are, at present, still usual in the Arabic world. The ghubâr numerals were found in Spain among the Western Arabs. Their forms varied more or less but were substantially as shown

on the table (1). It will be seen that these ghubār numerals resemble our modern numerals much more closely than the Hindu numerals do, and are almost identical with the forms of the abacus numerals given in the BOETHIUS geometry. (2)

Hindu	١	٢	٣	٤	٥	٦	٧	٨	٩
Ghubār	١	٢	٣	٤	٥	٦	٧	٨	٩
Modern	1	2	3	4	5	6	7	8	9

The name Hindu numerals is quite clear. It simply indicates the origin and source, it acknowledges the well established fact that the Arabs learned them from the Hindus. Much less clear, however, is the meaning of the term ghubār and the origin of the ghubār numerals.

2. *Two Theories on the Origin of the Ghubār Numerals.* — The earliest Arabic documents containing the ghubâr numerals are two manuscripts of 874 and 888 A.D. The oldest definitely dated European document known to contain these numerals is a Latin manuscript written in 976 A.D. (3) « That GERBERT (930-1003) and his pupils knew the ghubâr numerals is a fact no longer open to controversy... It is probable that GERBERT was the first to describe these numerals in any scientific way in Christian Europe, but without the zero » (4). That, in

(1) For more information about the ghubār numerals and their various forms cf. SMITH-KARPINSKI, *The Hindu-Arabic Numerals*, p. 69; SMITH, *History of Matematics*, II, pp. 70-71, 74; table of numerals at the end of CANTOR, *Geschichte der Mathematik*, I^{4}; DE SACY, *Grammaire arabe*, plate VIII; WOEPCKE, *Mémoire sur la propagation des chiffres indiennes*, Paris, 1863, pp. 36, 49; *Encyclopaedia Britannica*, 11th ed., under *Numerals*, vol. 19, p. 867.—The ghubâr numerals were first made known to modern scholars by SILVESTRE DE SACY, who discovered them in an Arabic manuscript from the library of the ancient abbey of Saint-Germain-des-Prés; see his *Grammaire arabe*, I^{2}, Paris, 1831, p. 91, and plate VIII; SMITH-KARPINSKI, *ib.*, p. 66. Cf. also SARTON, *Introduction to the History of Science*, I, pp. 648-49; 663, 670.

(2) Cf. G. FRIEDLEIN, *Boetii de institutione arithmetica*, Lipsiae, 1867, p. 397.

(3) SMITH-KARPINSKI, *loc. cit.*, p. 137-38; SARTON, *loc. cit.*, p. 664.

(4) SMITH-KARPINSKI, p. 112-13; SARTON, p. 670.

last line, they are to be traced back to India, like the so called Hindu numerals of the Eastern Arabs is the common opinion. But who brought them from India to Muslim Spain, and at which time were they introduced? On this question there are two general theories : (5) « The first is that they were carried by the Moors to Spain in the eighth or ninth century, and thence were transmitted to Christian Europe. The second advanced by WOEPCKE (6), is that they were not brought to Spain by the Moors, but that they were already in Spain when the Arabs arrived there, having reached the West through the Neo-Pythagoreans ». The facts that support WOEPCKE's theory are : the ghubār numerals differed materially from the Hindu numerals and resembled the abacus numerals. It was customary with the Arabs to adopt the numerical system of the countries they conquered. They adopted the Greek numerals in use in Damascus and Syria, and the Coptic in Egypt, and so on entering Spain it was only natural for them to adopt the abacus numerals in use there. Whether these ghubār numerals belonged to the Hindu system and reached Spain through the Neo-Pythagoreans of Alexandria as early as *c.* 450 A.D., as WOEPCKE thinks, or whether, as BUBNOV's theory holds, they were derived from the ancient Roman-Greek symbols used on the abacus, it is not our purpose here to discuss, or to decide. (7)

II. — THE ARABIAN ABACUS

3. *The Meaning of Ghubār is Abacus.* — The Arabic word ghubār has the meaning « dust, sand, earth », and belongs to

(5) SMITH-KARPINSKI, pp. 63, 65 and *sqq.*

(6) *Mémoire sur la propagation etc.*, pp. 181 *sqq.* In favor of the Roman origin are also A. v. HUMBOLDT, *Kosmos*, Stuttgart, 1847, II, pp. 263, 454-55, and CHASLES quoted by HUMBOLDT, *ib.*

(7) Unfamiliar with the Russian language, in which BUBNOV's work is written, the writer knows of BUBNOV's theory only as much as SMITH-KARPINSKI, p. 65, and RUSKA, in *Islâm*, X (1920), p. 87, and in *Zur aeltesten arabischen Algebra*, p. 82, say about it. There is a German translation of BUBNOV's work by J. LEZIUS under the name *Arithmetische Selbstaendigkeit der europaeischen Kultur*, Berlin, 1914, quoted by RUSKA, *ib.*, but the writer was unable, as yet, to get a hold of it. The fact of the « publicans' knots », discussed hereafter in § 6., seems to support BUBNOV's theory. I obtained BUBNOV's book while reading the last proofs and hope to discuss it in a second article.

the same family as the Arabic and Hebrew *'afar*, Assyrian *epru*, Tell Amarna *ḫaparu* and aparu, all denoting « dust, earth ». The term *ḫurûf-al-ghubār*, « the dust letters », or « dust numerals », was therefore explained by the later Arabic commentaries as having its origin in the ancient custom of the Hindus of writing the figures in the sand and performing their calculations on a board covered with sand. (8) This explanation was generally accepted by the European historians, among whom the opinion prevailed that the significance of the term ghubār is that the numerals were written on the sand, or on the dust board. (9)

In 1927 the writer first expressed his opinion (10) that in the phrases *ḫurūf-al-ghubār* and *hisāb-al-ghubār* the word ghubār does not simply mean « dust », or « dust board », but is the Arabic term for the abacus in general. It was pointed out that the Arabs not only knew the abacus, but that their terminology also reflected the double etymology of the Greek word *abákion*, namely as « table, tray », and as « dust » from the Hebrew *abaq*. (11) While the Eastern Arabs rendered it with the Persian term *takht*, « board, tray », the Western Arabs called it *ghubār* and sometimes also *turāb*, both meaning « dust, sand ». There is no more reason to restrict the ghubār to the dust abacus alone than we have reason to restrict the Greek-Latin term abacus to the dust abacus alone.

This assumption, of course, lends a strong support to WOEPCKE's theory. The ghubār, originated in Spain, is not an original Arabic term alluding to the Hindu custom of writing on the sand, but it is an attempt to arabicise the Roman term abacus. When the Arabs arrived in Spain, they learned to know the abacus and the abacus numerals; thus they rendered the word abacus by ghubâr, and the numerals found on the abacus were called *ḫurūf-al-ghubār*, the abacus numerals. This well accounts for the fact that the ghubār numerals are identical with characters

(8) See WOEPCKE, *loc. cit.*, pp. 34-42.

(9) See CANTOR, I^{4}, p. 712; SMITH, *History of Mathematics*, II, pp. 73, 175.

(10) In the article « Did the Arabs know the Abacus? », *The American Mathematical Monthly*, vol. 34, pp. 308-16, and especially pp. 314-15.

(11) In Hebrew the ghubār numerals are called *sūrōt ha abaq*, the figures of the dust, or also *mispar ha 'afar* and *mispar ha abaq;* see STEINSCHNEIDER, *Hebraeische Uebersetzungen*, pp. 558, 400 and 397, note 196; *The American Mathematical Monthly*, *ib.*, p. 315, note 2.

of the abacus as transmitted in the BOETHIUS geometry. The writer's theory regarding the Arabian abacus thus consists of two elements : 1. Ghubār means abacus; 2. It arabicises the Latin term abacus, which is conceived of as meaning dust. In the following some new facts will be advanced further corroborating this theory.

4. *The Testimony of Rabbi Judah of Barcelona (c.* 1100*).* — The first part of the statement, the equation : ghubār=abacus, is confirmed by the testimony of Rabbi JUDAH OF BARCELONA. Rabbi JUDAH was a famous scholar, author of several treatises on the Talmud, and a contemporary and fellow citizen of ABRAHAM SAVASORDA, with whom he once had a controversy on the question whether it is permitted to pay attention to astrology. (12) In his commentary on the *Sefer Yeṣīrah*, (13) he says : « The ten spheres are compared to the ten fingers, because calculation and writing, *i.e.* the letters, can not be accomplished without the fingers. The fingers write and they also figure up to infinity (14), and in writing and figuring they have more power than the arm or the other limbs. Because it is the fingers that immediately accomplish the writing. And with them we also compute every computation, even without speaking even as the Arabs do it with the fingers of their hands, the computation of the ghubār (15), and they compute every computation with them. So is it also with all the crafts and arts, they could not be achieved without the fingers. » JUDAH BARṢELONĪ speaks of the fingers as doing the writing, the calculation on the abacus, and the other crafts and arts. Hence he does not refer to the writing on the dust abacus, nor to finger reckoning, but to the ***mechanical counter abacus***, on which people figure with their fingers without speaking. (16)

(12) See *The Jewish Encyclopedia*, VII, p. 340.

(13) Ed. HALBERSTAM, Berlin, 1885, p. 140 at the bottom.

(14) Instead of *'eṣ* read *qēṣ;* cf. p. 311, *ib.*

(15) The text of this sentence reads : *ubahēn ḥoshbīn kol ḥeshbōn wa'afillū blō dibbūr kmo she'ōsīn ha'arb'iyyīm b'eṣb'ot yedēhen ḥisāb al ghubār.*

(16) A little further, however, on p. 142, line 22, Rabbi JUDAH quotes another anonymous commentator, who clearly does refer to finger reckoning. This anonymous commentator says : « Although we express several figures, we *see* only our tongue, which is in the middle, that talks, and the ten fingers with which we express the figures. For he who knows to count by the fingers, is able to

5. *The Pulvis, or Pulvisculus.* — On the other side it can also be proved that the Romans, too, understood the term abacus, which was a Greek loanword, as meaning dust, or dustboard, and, therefore, coined the corresponding Latin term *pulvis*, or *pulvisculus*, to denote the same thing. In the Satires of PERSIUS (17) (34-62 A.D.) we read : *nec qui abaco numeros et secto in pulvere metas / scit risisse vafer*, « nor the man (rogue) who has the wit to laugh at the figures on the slab and the cones drawn in sand. » This is the translation of CONINGTON, generally accepted, and the meaning is : PERSIUS does not care for the man who has his jokes at philosophy and science, simply because he does not understand it. But the words *secto in pulvere metas* offer many difficulties. Why just conic figures? If the conic figures are drawn in the sand, it ought to read *sectas in pulvere metas*. The writer, therefore, thinks that *pulvis* is another synonymous term for abacus and *metas* are the synonym of *numeros*. We have to translate : who laughs at the numerals on the abacus and at the figures on the dustboard partitioned by columns. » The *metas* like the later apices (18) simply denote the numerals on the abacus.

This conception is fully corroborated by the following verses of WALTER OF SPEYER written in the year 983 (19) :

Inde abaci « metas » defert Geometrica miras
Cumque « characteribus » iniens certamina lusus

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count by the ten fingers of the hand ten thousand, which are one myriad, and by the ten fingers of the foot up to ten myriads, which is the end of numeration. Since no master of computation, like the manager, secretary, treasurer and geometer has the right to add upon a hundred thousand, which are ten myriads ». But it is clear that Rabbi JUDAH, himself, in the preceding passage, p. 140, does not think of finger reckoning, because there is no connection whatever between the *ḥisāb-al-ghubār*, the calculation on the abacus, and finger reckoning. And yet these two passages apparently have given rise to the entirely erroneous conception that finger recknoing and *ḥisāb-al-ghubār* are one and the same thing. Cf. STEINSCHNEIDER, in *Monatschrift fuer Geschichte und Wissenschaft des Judentums*, vol. 50, (1906), p. 212, in *Gesammelte Schriften*, p. 434, note 69, and in *Hebraeische Uebersetzungen*, p. 400, note 215.

(17) A. PERSIUS FLACCUS, *Saturae* text with English translation by J. CONINGTON, 3rd ed., Oxford, 1893, p. 30-31 (I, 131); cf. also F. VILLENEUVE, *Les Satires de Perse*, Paris, 1918, pp. 52-53.

(18) *Boethius geometry*, ed. FRIEDLEIN, p. 397, and p. 426, line 30; see hereafter, §§ 16, 19.

(19) Printed in B. PEZ, *Thesaurus anecdotorum*, II, 3, p. 42. Quoted also by CANTOR, I^4, pp. 851-52, and by BUBNOV, *Gerberti opera*, p. 204.

Ocyus oppositum redigens corpus « numerorum »
In digitos propere disperserat articulosque...
Tradidit et varias in secto pulvere metas.

« Geometry then fetches the strange *figures* of the abacus, and, starting the battle game with the *characters*, she quickly arranges in opposite the *numbers*, and scatters them properly in digits and articles... And she hands down the various figures on the abacus, partitioned in columns ». Here we see that *varias... in secto pulvere metas*, taken literally from PERSIUS, are the same as the *abaci metas*. They are also synonymous with the characters, numbers, digits and articles. They are by no means the geometric figures, since WALTHER OF SPEYER mentions the geometric figures in the same context as *planas figuras* and *superficies*, nor are they the lines of the abacus as CANTOR and BUBNOV, *loc. cit.*, think. Like in the Middle Ages, the abacus, being the main instrument of calculation, was taken as the representative of the science of mathematics. So CICERO (20) speaks of the *pulvis eruditus*, « the learned abacus », and APULEIUS (21) says : *sinon modo campo et glaebis, verum etiam abaco et pulvisculo te dedisses.* « If you had devoted yourself not only to the field and soil but also to the abacus and calculating board ». Abacus and *pulvisculus* are synonyma for mathematics like *campus* and *glaebae* for agriculture.

Hence ghubār is nothing else than an Arabic translation of the term abacus, even as *pulvis* is the Latin rendering of the same term. The term ghubār either follows the example of *pulvis* in rendering abacus, or it is a direct translation of *pulvis*. Both ghubār and *pulvis* mean originally « dust, dust board », reflecting the etymology of abacus, or the idea that, originally, it was only a dust board. In reality, however, they are the terms for the abacus in general, which in later times was commonly the counter abacus.

This again goes to corroborate the theory of WOEPCKE that the ghubār numerals were learned by the Arabs in Spain from the Roman abacus. As we today speak of Roman and Arabic numerals, simply indicating the origin and source, so the Arabs spoke of the Hindu and ghubār numerals, both terms only giving the origin of the numerals.

(20) *De Deorum Natura*, 2, 18.
(21) *Apologia*, 16, 426.

III. — THE ROMAN KNOTS; 'UQÛD AND ARTICULI

6. *The Roman Knots.* — While pursuing his inquiries into another problem (22), the writer stumbled upon some further facts that are apt to throw more light upon the question of the ghubār numerals. MOSES BEN MAIMON, the famous Jewish philosopher, who was born and spent the early part of his life in the Western part of the Dār al-Islām, informed us that in his time still « the symbols and figures employed for computation on the registers of the accountants » were called « the Roman knots », *al-'uqūd al-rūmī*. It is clear that in speaking of « the symbols and figures etc. » MAIMONIDES refers to the ghubār numerals commonly used among the Western Arabs. The significance of the term « knots », however, calls for further explanation. The writer first (23) thought of a connection with the so called « publican's knots » that were usual in Palestine under the Roman government at about 200 A.D. There were two sizes of those publican's knots. A large rope serving as a revenue book and remaining in the hands of the tax collector, and a great amount of very small knots, that were given out as receipts to the people who paid the duty. Later on, they found it more practical to write receipts, but the written receipts were also called publican's knots, because their signs, letters or numerals, imitated or substituted the original knots.

On the other hand, we find also that the term knots, in Arabic *'uqūd* and in Latin *articuli*, does refer to the numerals, but only to a certain class of numerals, that is, to the tens, or to the higher decimal classes in general, as distinguished from the units or digits. This fact would seem to antagonize the testimony of MAIMONIDES, saying that *all* the numerals were called knots, and in the paper quoted above (24) the attempt was made to explain these terms as the vestigial remnants of the ancient knot abacus, where the tens and hundreds were represented by certain knots.

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(22) « The Knot in Hebrew Literature », *Isis*, vol. 14 (1930), pp. 189-214; see especially pp. 210-213, 206-7.

(23) *Isis*, *loc. cit.*, pp. 210-12.

(24) *Loc. cit.*, pp. 206-207.

However, after a more thorough study of all the sources available, where the terms *'uqūd* and *articuli* occur, the writer arrived at conclusions considerably modifying his former opinion. The writer's opinion now is that the testimony of MAIMONIDES regarding the Roman knots is in perfect harmony with the terminology of the *'uqūd* and *articuli* in their original meaning, both supporting each other and both confirming the theory of WOEPCKE. This testimony of MAIMONIDES contains two elements: 1. the *'uqūd* refer to the numerals in general; 2. they were called Roman *'uqūd*, hence they have a Roman origin, and the *articuli* are not the translation of *'uqūd*, as commonly believed, but their origin.

7. *Articuli and Finger Reckoning.* — But before we start to say what the articuli really are and mean, it will be fitting to make clear what they are not. It has been stated already (25) that the terminology of *digiti* and *articuli* has nothing to do with finger reckoning, or finger symbolism. The latter theory was originally set forth by CANTOR (26). The fact, however, is that in the extant works on finger reckoning (27) the joints were not confined to the expression of the tens and hundreds only, nor did the straight fingers represent the units. The custom was to represent the units by certain bendings of the little, ring- and middle finger of the left hand, while the tens were symbolized through bending of the two other fingers of the left hand. Similarly, the hundreds and thousands were expressed by the same positions given to the fingers of the right hand. In the Arabic sources the order is reversed. The units and tens are presented on the right hand, and the hundreds and thousands on the left hand (28). But in the Arabic as well as in the Latin sources of finger reckoning the principle of division is not between fingers and joints, or between units and tens. All the numerals from 1-10,000 are represented by the bending of the fingers, hence finger reckoning is called *ḥisāb-al-'uqūd*, and *'uqūd* here is in the meaning of *flexus*,

(25) *Isis, loc. cit.*

(26) *Mathematische Beitraege zum Kulturleben der Voelker*, p. 209.

(27) The best presentation of the subject with several illustrations is given in SMITH, *History of Mathematics*, II, pp. 196-202. See also CANTOR, I^4, p. 514 *seq.*, and for the Arabic sources cf. RUSKA in *Islām*, X (1920), pp. 87-119; see also *ib.*, p. 243.

(28) See *Islām, loc. cit.*

or *inflexiones, digitorum* and has nothing to do with the *'uqūd* referring to the numerals. In the system of finger symbolism it would be much more proper to distinguish between the three-finger (29) and the two-finger-numerals, or also between the right hand and left hand numerals, as was well pointed out by RUSKA (30). In the system of finger symbolism the term *'uqūd* refers to the primitive phase of digital notation, while the *'uqūd* corresponding to the *articuli*, refer to the numerical notation of a much later time. The one term refers to the digital language of manual or mechanical counting, the other one to the numerical signs of written mathematics.

A. THE 'UQŪD IN THE ARABIC SOURCES

8. *Al-Māridīnī (c. 1500).* — We have now to consider the apparent antagonism between the testimony of MAIMONIDES and the generally accepted theory concerning the *'uqūd* and the *articuli* (31), and will fittingly begin with the tradition in the Arabic sources. The common opinion regarding the *articuli*, and consequently also the corresponding *'uqūd*, as denoting the tens or also the higher orders, is based upon the Latin sources and their terminology of *digiti* and *articuli*. The Arabic sources, however, studied independently, without being prejudiced by the Latin documents, suggest quite a different theory. Here, in the Arabic tradition, the great majority of the evidence is in favor of the conception of the *'uqūd* as referring to the nine numerical figures in general, representing the units as well as all the other orders.

There is an Arabic treatise on sexagesimal calculation by MUHAMMAD SIBTH AL-MĀRIDĪNĪ (d. 1527) translated by WOEPCKE (32), in which we read : « Sachez que les ordres élémentaires des nombres sont au nombre de trois : unités, dizaines et centaines, dont chacun comprend neuf nœuds (33) ... On assigne

(29) The units and hundreds. According to some versions, however, the thousands are represented by the three fingers.

(30) *Islām, ib.*, p. 118-19.

(31) See § 6.

(32) *Sur l'introduction de l'arithmétique indienne en occident*, Rome, 1859, pp. 67-69. See p. 54, *ib.*, on the date of the author.

(33) Nœud, knot, is the translation of *'aqd*, pl. *'uqûd*.

a chacun des ordres élémentaires neuf lettres (34), une a chaque nœud; et on en assigne une a mille... Chaque nœud des dizaines est employé avec toutes les unités, et chaque nœud des centaines avec toutes les unités et dizaines... Toutefois le rapport d'un nœud quelconque d'un ordre quelconque au nœud correspondant de l'ordre suivant est là (35) un dixième, et ici (36) le sixième d'un dixième, parce que la limite de tous les nœuds est là neuf et ici cinquante neuf ».

Thus we see it here clearly and unequivocally stated that each of the three orders, the units, tens and hundreds, contains nine *'uqūd*, knots or *articuli*. It is true, AL-MÂRIDÎNÎ refers to the so called *ḥurūf-al-jumal*, the alphabetical numerals, but there is no reason to assume that the term does not apply to the ghubār numerals as well. Indeed, from the other instances it appears that originally the term was employed for the ghubār numerals only. It is further true, AL-MÂRIDÎNÎ lived as late as c. 1500, but he certainly used the term in accordance with the old established tradition. Otherwise, he would have expressly said that it is a new terminology that is being introduced by him.

9. *Al-Bīrūnī (c. 1000).*—In fact we find the term as being used in the same significance by AL-BĪRŪNĪ. So he says in his book on India : (37) *Wamimmā ittafaqa alayhi jami'u al-'umam fi-l-ḥisāb hūwa tanāsub* « *'uqūdihi* » *alā-l-a'shār. Famā min martabat fīhi illa wawāḥiduha 'ushr wāḥid allati ba'duhā wa'asharat aḍ'āf wāḥid allati qabluhā.* « There is one thing, on which all the races are agreed in their system of numeration, and this is the decimal proportion in which all its « knots » stand to each other. So that in each order the one is the tenth part of the following and the tenfold of the preceding ». SACHAU, in his English translation, rendered both Arabic terms, *'uqūd* and *martabat*, by the same word « order », which is not correct altogether. *Martabat*, « order, class », refers to the whole class of the units,

(34) AL-MĀRIDĪNĪ speaks of the Arabic alphabetical numerals, not of the Hindu numerals, hence he has different letters for the nine units, tens and hundreds and only one letter (the *ghain*) for one thousand.

(35) In the decimal system. The same idea in the same terms is also expressed by AL-BĪRŪNĪ (c. 1000); see § 9.

(36) In the sexagesimal system.

(37) *Alberuni's India*, ed. SACHAU, Arabic text, p. 83, lines 3-4; English translation, I, p. 174.

tens etc.; *'uqūd*, « knots », refers to the single members, or numbers, of the class. This appears unequivocally from the corresponding passage of AL-MĀRIDĪNĪ : « Toutefois le rapport d'un nœud quelconque d'un ordre quelconque au nœud correspondant de l'ordre suivant est un dixième ». It is also evident that the knots in AL-BĪRŪNĪ's sentence refer to all the orders, including the units, or, as has been said, they denote the nine numerals in general.

10. *The Theory and Testimony of Woepcke.*—In all this the writer's conception is supported by WOEPCKE, who quoted this passage of AL-BĪRŪNĪ in his *Mémoire sur la propagation des chiffres indiennes* (38) and remarked : « Les nœuds des unités sont 1, 2, 3, 4, 5, 6, 7, 8, 9; les nœuds des dizaines, 10, 20, 30, 40, 50, 60, 70, 80, 90; les nœuds des centaines, 100, 200, 300, 400, 500, 600, 700, 800, 900; et ainsi de suite. *C'est ce qui résulte pour moi d'un examen de nombreux traités d'artihmétique arabe* quoique ce ne soit pas tout à fait conforme à ce qu'on trouve dans la *Grammaire arabe* de M. DE SACY, deuxième édition, I, p. 417 ».

There is nothing to be added to that. It is only to be regretted that WOEPCKE did not quote in full the Arabic treatises. Still, his testimony is certainly reliable and not to be questioned.

11. *Al-Khuwārizmī (c. 825).*—The confusion arises in the terminology used in the *Algebra* of AL-KHUWÂRIZMÎ (39). Here the term *'aqd* occurs twice. In one instance it corroborates the theory of WOEPCKE, adopted by the writer, in the second one, it seemingly contradicts it. Confirmed is the theory by the passage that reads (40) : *Thumma kadhālika yuraddad al-'alf 'inda kulli 'aqdin ilā ghāyat al-madrak min al-'adad.* « Then the thousand is repeated in the same way *with each knot* to the utmost limit of numeration ». AL-KHUWĀRIZMĪ wishes to say : While the one, ten and hundred were repeated with the units only, or with the knots of the first order only, the thousand is repeated with *each* knot, from the first order of the units up to infinity (41).

(38) P. 95 *seq.*; see note 2 *ib.*

(39) Ed. F. ROSEN, London, 1831.

(40) *Algebra*, p. 3 of the Arabic text; p. 5 of the English translation.

(41) The Arabic language has different names for the first four orders only; hence the word thousand must always be repeated, when the thousand is reached. A million is thousand thousands, ten millions are ten thousand thousands etc.

The most natural thing is therefore to understand *'aqd* as a term including the units, too (42).

It is true, the Latin version of this passage given in *Algoritmi de numero Indorum* (43) has no equivalent for the phrase *'inda kulli 'aqdin*. But the reading is secured by both Latin translations. GERHARD OF CREMONA (44) has *apud unumquemque articulum* and ROBERT OF CHESTER (45) renders it by *secundum ordinem numerorum*. The latter rendering, of course, is not correct, since *ordo* means « order, class », and *'aqd* applies to each number of the class, as has been stated above (46).

Seemingly militating against our theory is the other passage (47) : *Fa'idhā kānat 'uqūd wama'ahā aḥād... falā budda min ḍarbihā arba'u marrātin, al-'uqūd fī-l-'uqūd wa-l-'uqūd fi-l-aḥād wa-l-aḥād fī-l-'uqūd wa-l-aḥād fi-l-aḥād.* « If there are knots and units, or the units are subtracted from them, then four multiplications are necessary : the knots by the knots, the knots by the units, the units by the knots and the units by the units ». Thereupon AL-KHUWÂRIZMÎ continues by illustrating this rule by the cases (10 + 1) (10 + 2), (10 — 1) (10 — 1), (10 + 2) (10 — 1), (10 + X) (10 + X) etc., where always ten is the first number standing for the knots. Hence the most natural conclusion would seem to be that the *'uqūd* denote the tens only, or, if we do not press the ten but take it as representing the non-units, that they denote the higher orders outside of the units. In the face of the contradicting evidence, however, we might try to interpret it, that the knots in general denote any numeral of any order, but while speaking of « knots and units », the knots refer to any numeral standing outside of the units. It is readily admitted that such an *interpretation* sounds a little unnatural and forced, doing violence to one fact in favor of a theory. But otherwise, interpreting the *'uqūd* as meaning the tens only, we would have to do violence to the great majority of the facts in favor of a theory still less

(42) Cf. hereafter, § 13, note 63.

(43) In *Trattati d'Aritmetica*, ed. BONCOMPAGNI, Roma, 1857, p. 3, where AL-KHUWÂRIZMÎ is quoting his algebra; cf. RUSKA, *Zur aeltesten arabischen Algebra*, p. 74.

(44) Ed. LIBRI, I, p. 254, line 3.

(45) Ed. KARPINSKI, p. 66.

(46) § 9.

(47) Arabic text, p. 15; English translation, p. 21 *seq.*

tenable. So nothing remains than to either admit our ignorance in the explanation of this one case, or else to say that, originally, there were two different sources using the term *'aqd* in two different meanings. In one it was used to denote any numeral, and in the other one, like in some of the Latin sources, it was employed to signify the tens. AL-KHUWĀRIZMĪ, in his eclectic and rather careless manner familiar to us from other passages (48), uses this term, like many other terms, in different meanings.

12. *The General Confusion. Woepcke Retracts.*—This passage, or also other ones like that, apparently was it that gave rise to the general confusion regarding the *'uqūd* (49). So the Arabic dictionaries, *Asās* and *Qamūs*, quoted by LANE (50) say *al-'asharat awwalu-l-'uqūd* « the ten is the first knot », thus representing the conception that the *'uqūd* are the tens only. S. DE SACY in his Arabic grammar (51) translates *'uqūd* by « nœuds », but he remarks : « Ce sont les noms des dizaines, depuis vingt jusqu'à quatre-vingt-dix ». So he starts the *'uqūd* with twenty, which has its parallel in some of the Latin glosses to be discussed hereafter (§ 17). ROSEN, in his English translation of the *Algebra*, renders the word once (on p. 5) as « complex number » and then again (on p. 21) with « greater numbers ». In his notes (p. 187) ROSEN says that AL-KHUWĀRIZMĪ used the term in both passages as a general expression for all numerals of a higher order than the units. SUTER, in a private letter to ENESTROEM (52), says that the term on p. 3 designates all the multiples of ten, while on p. 15 it stands for the tens only. The Latin translators render it with *nodus* and *articulus* (53). RUSKA (54) ends his discussion with an honest *non liquet*. He says : « Die von ENESTROEM gewuenschte naehere Untersuchung ueber das Auftreten der Bezeichnung *articulus* kann zu keinem Ergebnis fuehren, solange nicht die Geschichte des Ausdrucks auf arabischem Gebiet

(48) See the writer's « Studies in History of Mathematics » in *The Hebrew Union College Annual*, VI (1929), p. 249.

(49) See also hereafter, § 15.

(50) *Arabic-English Dictionary*, p. 2106 ; see also WOEPCKE, *Calcul Gobari*, p. 2, note.

(51) I (1831), p. 417.

(52) *Bibliotheca Mathematica*, IX_2 (1908), p. 350.

(53) See hereafter, § 15, note 91.

(54) *Zur aeltesten arabischen Algebra*, pp. 79-82.

festgestellt ist. Koennte man hier eine natuerliche Entwicklung nachweisen, die alle in den Schriften der Abacisten und Algorithmiker auftretenden Bezeichnungen umfasst, so waere auch die Hauptfrage nach der Herkunft der lateinischen Terminologie erledigt ».

Finally WOEPCKE, himself (55), became confused through this passage, gave up his original correct theory stated in 1863, and accepted an epxlanation given in FREYTAG's Arabic-Latin dictionary (56), which is untenable altogether. WOEPCKE was also misled by a passage in the *Introduction au Calcul Gobārī et Hawāī* (57) that offers some difficulties. It reads in the French translation : « Toujours la place du nombre indique son espèce, s'il est des unités ou de ce qui vient après elles, tandis que sa figure indique sa quantité (58)... Donc l'unité est la première dans chaque premier nœud, et la seconde est la première de chaque second nœud, et ainsi de suite ». WOEPCKE's suggestions for the explanation of this last crucial sentence are all mistaken. In the writer's opinion, however, there is only one way to explain it, and that is as follows : As we already know, each order, or row *(martabat)*, has nine knots *('uqūd)*, which are the nine numerals 1-9. It is only natural to call the one (1) the first knot, the two (2) the second knot, the three (3) the third knot, etc. Thus the first knot denotes the one standing in any row whatever, as 1, 10, 100, 1000 etc. Hence the figure one stands first in each first knot, and then the zeros follow, the figure two stands first in each second knot, etc. Thus this passage would also corroborate the original theory of WOEPCKE that the *'uqūd* stand for the nine figures and that there are nine *'uqūd* in each order.

B. — THE ARTICULI, OR THE LATIN TRADITION.

13. *Alcuin of York (735-804).*—The earliest Latin source

(55) See *Introduction au Calcul Gobârî et Hawâî*, Rome, 1866, p. 2, note.

(56) In FREYTAG's opinion the *'aqd* means a composite number, where two numbers are tied together, forming, so to say, a knot. This theory is obviously wrong and contradicted by *all* the Arabic and Latin sources.

(57) P. 2. The Arabic treatise was translated into French by WOEPCKE and edited after his death by BONCOMPAGNI in *Atti dell' Accademia pontificia de Nuovi Lincei*, 1866, The author and time of the treatise are unknown, but it was written after IBN AL-BANNĀ (c. 1300) and before 1573; see CANTOR, I^4, p. 816.

(58) That is : the coefficient.

mentioning the *articuli* is a letter of ALCUIN of York, « the greatest representative of learning and culture in the West » (59) in the eighth century. In epistle 259 (60) ALCUIN replies to a question asked by his disciple DAPHNIS : *quomodo in... cantico Salomonis sexagenarius numerus reginis conveniret et octogenarius concubinis?* Why in the *Song of Songs* (61), ascribed to King SOLOMON, there are threescore of queens and fourscore of concubines (62)? His answer is that six is a perfect number ($6 = 1 + 2 + 3$) and that sixty in the tens occupies the place of six in the units, hence it fits for the queens. The passage bearing on our subject reads : *Item progressionem numerorum articulis, quasi quibusdam unitatibus, ad infinita crescere per quasdam finitas formas videmus. Nam prima progressio numerorum est ab uno usque ad decem. Secunda a decem usque ad centum. Tertia a centenario numero usque ad millenarium. Et eandem regulam perfectionis ver imperfectionis, quam prima unitas in suis numeris sub denario servat, eandem et secundam in suis denariis sub centenario servare necesse est. Nam sicut senarius numerus in suis partibus per unitatem positis perfectus est in se ipso, ita et sexagenarius partibus suis positis per denarium perfectus esse dinoscitur, ita ut semper in sexagenario numero denarius numerus locum obtineat unitatis in senario.* « We see also that the progression of numbers through the articles, being, so to say, certain units, grows up to infinity by a limited number of certain forms. For the first progression of numbers is from one to ten, the second from ten to a hundred, and the third from a hundred to thousand. And the same rule of perfection, or lack of perfection, that prevails in the numbers of the first units, below the ten, must also obtain for the second units that are the tens standing below the hundred. Thus even as the number six, with its parts in the order of the units, is perfect, so also must the number sixty, with its parts in the order of the tens, be recognized as perfect, because the

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(59) SARTON, *Introduction to the History of Science*, I, p. 529.

(60) *Monumenta Alcuiniana*, ed. WATTENBACH and DUEMMLER, Berlin, 1873, p. 820.

(61) VI, 8.

(62) By the way it will be interesting to remark that the discussion of the same problem and various allegorical interpretations of it are also to be found in the *Midrashîm*, i.e. the collections of homiletical commentaries to the Bible written by the Rabbis of about 100-800 A.D. Cf. *Midrash Rabbah, Canticum*, VI, 8.

ten in the number sixty always occupies the same place as the one in the number six ».

By this passage we see clearly that the articuli are certain forms and certain units, or let us say rather : « the forms of certain units », by which the numbers are progressing up to infinity. This, however, is true only of the nine numerals of the Hindu system with place value. The main characteristic of the Hindu system being that we are able to express all the numbers by the nine numeral figures representing the units. Hence ALCUIN shows himself to be familiar with the Hindu system, and he calls the nine Hindu numerals in general, not only the tens, articuli.

Another main characteristic of the Hindu system is the place value. The underlying principle of it is the conception of all the higher decimal classes as mere reiterations of the first class of units. To this conception the tens, hundreds, thousands, etc. are after all also only units, but of a higher or secondary order. In the earliest expositions of the Hindu system by AL-KHUWĀRIZMĪ these two characteristics are stressed (63), and here, too, we find ALCUIN of York stressing the point that the units are *prima unitas*, while the tens represent *secundam unitatem*, and that the ten in the sixty occupies the place of the one in the six. Hence the assumption is reasonable that ALCUIN was familiar with the Hindu numerals, to which he refers by the name of articuli.

Moreover, in a poem written between 780 and 796, or probably even to be dated between 780 and 782 (64), ALCUIN praises EGBERT that he was teaching *diversas numeri species variasque figuras*, « different kinds and various figures of the numbers ». In the *species numeri* we might see an allusion to a passage in BOETHIUS (65) reading : *Paris autem numeri species sunt tres*, and the *varias figuras* remind of the *varias in secto pulvere metas*, quoted above (§ 5) and of the *diverse formatos apices vel caracteres* mentioned in the BOETHIUS geometry (66).

(63) Cf. the *Algebra*, quoted above § 11, where it is said that it is by means of the knots that the thousand is repeated up to infinity. See also *Algoritmi de numero Indorum*, pp. 2-3.

(64) It is verse 1445 on p. 125 of *Monumenta Alcuiniana* ed. WATTENBACH. On the date see WATTENBACH, *ib.*, p. 80, and CANTOR, I^{4}, p. 840. It was CANTOR who called the attention to these passages of ALCUIN.

(65) Ed. FRIEDLEIN, p. 17, line 1.

(66) P. 397, line 2.

ENESTROEM originally thought (67) that the term articulus was not used before the thirteenth century. Then he was informed by SUTER (68) that the corresponding Arabic term occurs already in AL-KHUWĀRIZMĪ's algebra, whereupon he modified his theory to the effect that the first algorithmists took the term from Arabic sources, and suggested that the question be further investigated. Now we see that not only the term articulus, but also a surprizing familiarity with the Hindu system is to be found in the works of ALCUIN. The famous ecclesiast lived in England and France from 735-804, while AL-KHUWĀRIZMĪ wrote his book at Bagdad under the reign of the Caliph AL-MĀMŪN (813-33). So the assumption of a direct connection between ALQUIN and AL-KHUWĀRIZMĪ would be highly improbable. The possibility of an influence exerted by earlier Arabic sources is also very slight. ALCUIN's works are written in the style and spirit of the church fathers, mingling the elements of classic and Hebrew-Christian culture, but entirely free from any Arabian influence.

It is true that at the time of ALCUIN and his royal friend CHARLEMAGNE there were some merchants, travellers and emissaries passing back and forth between the East and West, « and with such ambassadors must have gone the adventurous scholar, inspired, as ALCUIN says of Archbishop ALBERT OF YORK (766-780), to seek the learning of other lands (69). « There is also a cruciform brooch in the British Museum inlaid with a piece of paste on which is the Mohammedan inscription, in Kufic characters, ' There is no God but God ' » (70). How did such an inscription find its way, perhaps in the time of ALCUIN, to England? And if these Kufic characters reached there, why not the numeral forms as well? » So ask SMITH-KARPINSKI. Similarly, RUSKA (71) thinks only of two possibilities : either ALCUIN invented the term articulus, or he learned it from the Moors. He says : « Sollen wir nun annehmen, dass ALCUIN den Ausdruck selbst erfunden hat, oder ist es denkbar, dass schon zu seiner Zeit von Spanien her Funken

(67) *Bibliotheca Mathematica*, $VIII_2$ (1907), pp. 150-51.

(68) *Bibl. Math.*, IX_3 (1908), p. 350.

(69) SMITH-KARPINSKI, *The Hindu-Arabic Numerals*, p. 103.

(70) *Ib.*, pp. 101-102, but no further source is given. Cf. also WOEPCKE, *Mémoire sur la propagation*, p. 147, note 1.

(71) *Zur aeltesten arabischen Algebra*, p. 81.

arabischer Wissenschaft nach dem Frankenreich flogen? » In the writer's opinion, however, there would be more probability for the assumption, that some of those emissaries, pilgrims and scholars came in touch with the Nestorian priests of Syria (72), who, like SEVERUS SEBOKHT, were familiar with the Hindu numerals as early as 662 (73). Moreover, « it is not beyond the range of possibility that proof may sometime come to light to show that the Greeks and Romans knew something of the number system of India, as several writers have maintained », say SMITH-KARPINSKI (74). If the writer's interpretation of the verse in the Satires of PERSIUS (75), *qui abaco numeros et secto in pulvere metas*, will be accepted, then indeed this proof did come to light. Then indeed we might be justified in the assumption that ALCUIN, BOETHIUS (76) and PERSIUS had the knowledge of certain forms of the ghubār, or abacus numerals without the zero. Then indeed it is quite possible that BUBNOV is right in his theory of the arithmetical independence of the European nations (77).

14. *Gerbert (930-1004).*—In later times, however, the terminology of the *digiti* and *articuli*, where the digiti stand for the units and the articuli for the tens, became so common and predominant that it entirely crowded out the more ancient usage, still preserved in ALCUIN's works, which came altogether in oblivion. This later terminology is first traceable to the mathematical works of GERBERT, also known as pope SYLVESTER II (999-1004), and his contemporaries. Here we can still see the original significance of this terminology and how the necessity arose of creating a new artificial terminology in spite of the existence of such plain and natural terms as *singulares* and *unitates* for the units and *deceni* and *denarii* for the tens.

So we read in GERBERT's *Regulae de numerorum abaci rationibus* (78) : *Si multiplicaveris singularem numerum per decenum dabis*

(72) Or with their books.

(73) This was first discovered by NAU in *Journal Asiatique*, 1910, p. 225. See also SMITH, *History of Mathematics*, I, p. 166-67; RUSKA, *loc. cit.*, p. 45-46; SARTON, *Introduction*, p. 493.

(74) *Loc. cit.*, pp. 80-81.

(75) See above § 5.

(76) Even if the passage in his geometry is admitted to be a falsification.

(77) See above, § 2 at the end.

(78) N. BUBNOV, *Gerberti Opera Mathematica*, Berlin, 1899, pp. 8-11.

unicuique digito decem et omni articulo centum. Si decenum per decenum, dabis digitis centum, articulis mille. Si decenum per centenum, dabis digitis mille, articulis decem millia, etc. « When you multiply a unit with one of the tens, then each digit belongs into the column of the tens and each article into the column of the hundreds. When you multiply one of the tens with one of the tens, then the digits stand among the hundreds and the articles among the thousands. When one of the tens with one of the hundreds, then the digits stand among the thousands and the articles among the ten thousands, etc. » The parallel text of another manuscript, supposed to be an interpolation (79), reads in the beginning : *Si multiplicaveris singularem numerum per singularem, dabis unicuique digito singularem et omni articulo decem.* « When you multiply a unit with a unit, then each digit belongs into the units and each article into the tens ». GERBERT teaches the rules of the multiplication on the abacus by multiplying the single ciphers with each other. The regular names : *singulares, deceni, centeni, etc.*, are used to signify the *absolute values of multiplier and multiplicand*. The results of the multiplication of the single ciphers 1-9 lie between 1 and 81, they consist only of units and tens, but they are *absolute* units and tens only when the two factors are units. When the factors are, f.i., tens, then the results are hundreds and thousands. Thus the digits and articles are only *relative values*, standing to each other in the relation of one to ten, but in reality representing all the numbers possible. The old natural terminology of the *singulares, deceni, etc.*, was used for the *absolute values of the two factors*, while the new terminology of the digits and articles was introduced to denote the *relative values of the product*, having the proportion of one to ten.

This meaning and significance of the digits and articles will be found in the mathematical writings of GERBERT's contemporaries (80) too. As f.i., in the commentary of ABBO OF FLEURY (d. 1004) on the *Calculus* of VICTORIUS (c. 450) (81), and in the *Regulae abaci* of HERIGER OF LOBBES (82).

(79) See BUBNOV, *ib.*

(80) Collected by BUBNOV in his edition of GERBERT's works.

(81) BUBNOV, *ib.*, pp. 197-203; see also SARTON, *Introduction*, I, p. 671.

(82) BUBNOV, *ib.*, pp. 205-25; SARTON, *ib.*, p. 685.

This explanation was clearly recognized and expressed by some of the earlier commentaries to GERBERT. The one says : (83) *Digiti autem nuncupantur minores numeri ex qualibet multiplicatione concreti, qui videlicet intra denarium constituti... Articuli autem appellantur maiores numeri, similiter ex aliqua multiplicatione producti... Nam multiplicatores et multiplicandi nec non et dividendi appellantur et singulares, et deceni, et centeni, et milleni et deinceps secundum dispositionem numerorum abaci. Notandum autem, quod in omni multiplicatione numerorum primo quidem constituendi sunt digiti in singularibus et articuli in decenis, postmodum vero secundum certam regulam certis distributi sunt intervallis.* Part of this definition is repeated in another commentary (84).

This relativity of the *digiti* and *articuli* is also very clearly stated by GERBERT himself. He says : (85) *Quid cum idem numerus modo simplex, modo compositus, nunc digitus, nunc constituatur ut articulus?* « How is it that the same number is sometimes simple and sometimes a composite, now a digit and then an article? » What he means to say is most probably, that, *e.g.*, in $4 \times 6 = 24$, the number 2 (= 20) is the article and the number 4 is the digit, while in $7 \times 60 = 420$, we figure $7 \times 6 = 42$, and the same 2 (= 20) is a digit, whereas the 4 (= 400) is the article (86).

It will now be well understood why the *regulae abaci* usually stress the importance of knowing what the *digiti* and what the *articuli* are (87). Because the main difficulty in the whole art of multiplication consists in the right knowledge of the nature of the digits and articles and of their assignment to the proper columns of the abacus. On the other side, were the digits and articles only the absolute values of the units and tens as they

(83) BUBNOV, *ib.*, pp. 252-53.

(84) See BUBNOV, *ib.*, p. 269, note 1.

(85) BUBNOV, *ib.*, p. 7.

(86) BUBNOV, *ib.*, note 12, did not understand this passage. He interprets it that the number 4 in 4 and 40, being simple numbers, is a digit, while in 42, being a composite, it is an article. This is not true, because the 4 in 40 is an article, as is also stated in the commentary, *ib.*, p. 252, lines 24-26. *Modo simplex modo compositus* is to be explained with BUBNOV, that the 4 in 40 is simple and in 42 is composite. But this sentence is not to be connected with *nunc digitus, nunc articulus* to mean that the *simplex* is always a *digitus* and the *compositus* always an *articulus*.

(87) GERBERT in ed. BUBNOV, *ib.*, p. 7, line 2; the BOETHIUS geometry, p. 395; *Joannis Hispalensis Liber Algorismi*, p. 25, lines 9-10.

occur in the factors, then there would be no reason for attaching such a great importance to their correct understanding, as indeed there would be no reason for their introduction at all.

The question as to whether or not GERBERT studied under Arabian teachers, or was under the influence of Arabic science is not yet decided. We know that he visited Spain in about 967 and stayed there for three years, but his stay there was confined to the country of Barcelona, then entirely under Christian rule. So far, however, there is no direct evidence in favor of his being in touch with Arabian culture, nor is there any evidence against it (88). The terminology of the *digiti* and *articuli* as used and understood by GERBERT has nothing to do with the Arabic terminology of the *'uqūd* and *aḥād*, as used by AL-KHUWÂRIZMÎ (89), because the *'uqūd* and *aḥād* there are simply the tens and units, as the absolute values of the factors, corresponding to the *singulares* and *deceni*.

15. *The Latin Translations from the Arabic. Beginning of the Confusion.*—As a matter of fact, it seems that the confusion first started with the Latin translations of this very passage of AL-KHUWĀRIZMĪ (90). The terms *'uqūd* and *aḥād* were translated by GERHARD OF CREMONA (91) (c. 1150) with *articuli* and *unitates*. Hence these translators and their readers thought that the *articuli* denote the absolute tens of the factors, and consequently the *digiti* were also understood as representing the absolute units.

So we read in the *Liber Algorismi de pratica arismetrice* (92):

(88) Cf. SMITH-KARPINSKI, *loc. cit.*, pp. 111 *seq.* and 115 *seq.*; CANTOR, I^{4}, pp. 848 *seq.*

(89) See § 11.

(90) See above § 12.

(91) Ed. LIBRI, p. 265. The phrase *'inda kulli 'aqdin* of the *algebra*, p. 3, is also rendered with *apud unumquemque articulum*; see above § 11. ROBERT OF CHESTER, ed. KARPINSKI, p. 90, translates *'uqūd* and *aḥād* with *nodi* and *unitates*, while *'aqd* of p. 3 is given by *ordo*; see § 11.

(92) P. 116. The book was edited by BONCOMPAGNI in *Trattati d'Aritmetica*, Roma, 1857, as the continuation of the *Algoritmi de numero Indorum*, of which it is an adaptation by an anonymous author. The inscription : *Joannis Hispalensis Liber Algorismi etc.*, does not yet prove that JOHN OF SEVILLE (c. 1140) is really the author of the book; see ENESTROEM in *Bibliotheca Mathematica*, VI$_3$ (1905), p. 114; IX$_3$ (1908), pp. 2-4; CANTOR, I^{4}, pp. 800 *sqq.*—The adaptation of the *Algoritmi de numero Indorum* reaches only till p. 93. From p. 93 to the end various fragments and excerpts of unknown authors are given. On p. 93 a new treatise

Si articulum in articulum, vel digitum in digitum, vel compositum in compositum multiplicare volueris, figuram per figuram multiplica. Deinde numeros denominantes differentias agrega; ab agregato unum subtrahe, et principium sequentis differentie ab eo quod remanet denominate tociens excrescit, quot unitates excreverint ex multiplicatione figurarum; et quot excreverint denarii, tociens principium sequentis differentie excrescit (93). Here the terminology is used in just the opposite way. The absolute values of the factors are called *digiti* and *articuli*, and the relative values of the product are called *unitates* and *denarii*. In the same sense the terms *articuli* and *digiti* are used almost throughout the second part of the book (94). But, as has been stated (95), this book contains several parts, fragments and glosses belonging to different authors, hence some modifications in the meaning of this terminology will be found there, which will be discussed hereafter.

16. *The Boethius Geometry. A Further Element of Confusion.*— Different hands with contradicting conceptions will also be found in the famous passage of the so called BOETHIUS geometry, dealing with the abacus (96). On p. 398 the terminology is used in the ancient and correct sense as given by Gerbert. It reads : *Scire autem oportet et diligenti examinatione discutere in multiplicando et partiendo, cui paginulae digiti et cui articuli sint adjungendi. Nam singularis multiplicator deceni digitos in decenis, articulos in centenis, idem vero singularis multiplicator centeni digitos in centenis, articulos in millenis et multiplicator milleni digitos in millenis et articulos in decenis millenis et multiplicator centeni milleni digitos in centenis millenis, articulos autem in millenis milibus habebit, etc.*

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begins with the words *Hic incipiunt regule*. On p. 112 some fragments begin with the inscription *Exceptiones de libro, qui dicitur gleba mutabilia (i.e. : aljabr wa-l-muqabalah)*. The book in all its parts is under Arabic influence.

(93) *Numeros denominantes differentias* are the exponents of the orders indicating the column, or value, of a figure. The units have the exponent 1, the tens 2, the hundreds 3 etc. The rule for finding the value of the product is : sum of the exponents of the factors minus one.—*Principium sequentis differentie* means : « the first number of the resulting order », which is to be taken as many times as the coefficient of the units or tens indicates.

(94) See pp. 116-122; 132-135; especially p. 120, lines 19-20; p. 121, lines 8, 15-16; p. 122, lines 1-2.

(95) See note 92.

(96) Ed. FRIEDLEIN, pp. 395-400. On the BOETHIUS question see SMITH-KARPINSKI, *loc. cit.*, pp. 70 *sqq*.

It will be seen that this passage is in full accord with the *Regulae Multiplicationis* of GERBERT (97). Evidently from another hand, however, comes the definition on p. 395. Here the falsificator, too, begins with the words : *Nosse autem hujus artis dispicientem, quid sint digiti, quid articuli... oportet*. But apparently, he himself, did not understand the right meaning of the terminology, and besides, a new element of confusion was added by what seems to be a later gloss, inserted in the text. The definition here reads : *Digitos vero, quoscunque infra primum limitem, id est omnes, quos ab unitate usque ad denariam summam numeramus, veteres appellare consueverunt. Articuli autem omnes a deceno in ordine positi et in infinitum progressi nuncupantur*. « As digits the ancients used to designate all the numbers below the first limit (98), i.e. all the numbers from one up to the sum of ten (99). As

(97) Quoted above § 14. See BUBNOV, *ib.*, pp. 8-11, and p. 160, note 23.

(98) The falsificator identifies *limes* with *articulus*. To him the first limit is ten and the second limit is twenty etc.; cf. lines 12-16 of the same page : *Compositi quippe numeri sunt omnes a primo limite id est a decem usque ad secundum limitem id est viginti... Incompositi autem sunt digiti omnes annumeratis etiam omnibus limitibus*. Since the *articuli* in his definition represent also the higher orders, the same, in his opinion, must obtain also for the *limes*, as indeed appears from the words : *annumeratis omnibus limitibus*. This conception, of course, is wrong altogether. The right conception is found in the BOETHIUS geometry, p. 426, lines 28-30 : *et deinceps ceteras lineas ceterorum numerorum limitibus limitavimus*, and also in the *Liber Algorismi de pratica arismetrice*, where *limes* is = *ordo* = *differentia*, that is to say, the columns of the abacus, or the positions of the Hindu numerals with place value. Here the first limit is the column of the units, the second limit is the column of the tens, the third limit is the column of the hundreds etc.; cf. *Liber Algorismi*, p. 27, and hereafter, note 104. This usage of the term *limes* is also found in the works of the later algoritmists; see SMITH, *History*, II, p. 13. In the later parts of the *Liber Algorismi*, however, we find again the identification of the *limes* with the *articulus* like in the BOETHIUS geometry; cf. p. 116, at the bottom, *articuli sive limitis, articulum vel limitem;* p. 117, lines 12 and 16, *articulo vel limite, articulum vel limitem;* p. 118, lines 16 *sqq.*, always : *articulus vel limes*. This goes to corroborate the opinion of ENESTROEM, quoted above, note 92, that this treatise reaches only to p. 93, where another anonymous book begins with the words : *Hic incipiunt regule*. A somewhat fantastic use of the term *limes* is found in the *Algorismus* of SACROBOSCO (c. 1250); see ENESTROEM, *loc. cit.*, XI_2 (1897), pp. 97-102. On still another conception of *limes* see below, note 110.

(99) *Denaria summa*, « the summarizing number ten », or « the general number ten », seems to be of old Roman stock and is an interesting term. It indicates the classification of the numbers in « special, singular ones » for the units, and « general ones » for the higher orders. And indeed we find the term *singularis* used for the units a little later, p. 398, lines 12, 14; p. 400, lines 6 *sqq.*, and also

articles, however, all those were named which stand in the rows (100) beginning with ten and are progressing to infinity ».

This definition firstly misunderstands the digits and articles as absolute values. Secondly, it adds a further element of confusion by defining the articles as representing not only the tens, but also all the higher orders, or as equal to $a.10^n$, where a is smaller than ten. This conception, of course, has no basis or justification whatever. If we take the digits and articles, with GERBERT, as relative values, then both of them may, possibly, represent all the numbers up to infinity. If we take them, with the second passage in AL-KHUWĀRIZMĪ's algebra, as absolute values, then they stand for the units and tens only. Besides, the phrase : *a deceno in ordine positi et in infinitum progressi*, is even for a mediaeval writer too awkward.

In the writer's judgment, this confusion can be explained only by the corruption of the text. Originally, the text must have read : *articuli autem omnes deceno in ordine positi nuncupantur*. It represented the conception of the Latin translation from the Arabic, discussed in § 15, that the articles are the absolute tens. A later glossator, however, knew of the ALCUIN passage saying : *progressionem numerorum articulis... ad infinita crescere... videmus* (101), and in his scholastic way thought that it would be good to harmonize both, the BOETHIUS and the ALCUIN passage, so he added the marginal gloss « a » deceno in ordine positi « et in infinitum progressi », which gloss entered the text and caused the new confusion.

In this corrupt form the definition of the *articuli* was accepted by the author of the first part of the *Liber Algorismi* etc. He says (102) : *Ad habendam autem plene numerorum notitiam, oportet prenoscere, qui numeri dicantur digiti, qui articuli, quique compositi.* « In order to have a full knowledge of the numbers, one must first know, which are called digits, which articles and which composites ». Then he continues (103) : *Ordines vero sive limites*

regularly in the mathematical writings of GERBERT and his contemporaries; see § 14. Similar terms are the Talmudic *kelal* and *perat* ; see hereafter § 18, note 122.

(100) Ordo = differentia = « column, order »; see notes 98 and 104.

(101) See § 13.

(102) P. 25. Note the parallelism with the BOETHIUS passage in certain phrases. like : *oportet prenoscere etc., ordines in infinitum procedunt.*

(103) On p. 27.

numerorum, a primis numeris, qui digiti vocantur, et sunt 9., *per decuplos in infinitum procedunt... Omnes autem, qui sunt in ceteris limitibus, preter primum, articuli solent appellari.* « The orders, or the limits (104), of the numbers begin with the first numbers, which are called digits and of which there are nine, and progress up to infinity by their decimal multiples. But all the numbers standing in the other limits, after the first one, are usually designated as articles ».

In the same sense the term *articulus* occurs in many other treatises, e.g., in the *Liber algorizmi* edited by CANTOR, in the *Prologus Ocreati in Helceph*, in the *Algorithmus demonstratus* and in the *Algorismus vulgaris* of SACROBOSCO (105). Hence this definition of the term was also accepted by CANTOR, who says (106):

(104) Here we have the definition *limes = ordo ;* see above, note 98. The term *limes*, however, is here also used for *differentia* and denotes the column or position; see the same p., lines 1-14, where it is emphasized that the Hindus called the limits by the name *differentia*. The passage reads : *Notandum autem, quod singulis limitibus... indi dederunt nomina. Primum scilicet limitem... vocantes differentiam unitatum... Secundum autem... differentiam decenorum etc.* This goes to show that the author regarded *limes* as a Latin term, while *differentia* was known to be of Hindu-Arabic origin. See also TROPFKE, *Geschichte der Elementar-Mathematik*, I_2, p. 12 : « In den aeltesten lateinischen Schriften, die das indische Zifferrechnen nach dem Abendlande brachten, war Stelle mit *differentia*, nach arabischer Vorlage, wiedergegeben ». The explanation of the term *differentia* is as follows : In some of the Latin translations from the Arabic the chapters are called *differentiae*, thus rendering the Arabic *faṣl, fuṣūl ;* see STEINSCHNEIDER, *Die hebraeischen Uebersetzungen*, pp. 555, 568, 572, note 249. The Arabic word *faṣl* means : « incision, division, partition, section, separation », and also « difference ». The Latin translator chose the equivalent *differentia*, which, of course, is the wrong thing. The right translation would be « incision, division, section » etc. The Latin term *differentia* for the columns, lines, or sections on the abacus suggests that the original Arabic term was here, too, *faṣl, fuṣūl*, which in its meaning « incision, section » etc. is indeed well fitting. The Latin translator, however, committed the same error as in the term for the chapters, and rendered it by *differentia*. This explanation was suggested by a marginal gloss of STEINSCHNEIDER. The writer used the STEINSCHNEIDER copy of the *Algoritmi de numero Indorum*, extant in the Library of the Jewish Theological Seminary of America, where the whole collection of the STEINSCHNEIDER books is to be found. In this copy there is, on pp. 3 and 133, a marginal gloss in the handwriting of STEINSCHNEIDER rendering the word *differentia* by *faṣl* and referring to the passages of *Hebraeische Uebersetzungen*, quoted above.

(105) All quoted by ENESTROEM, *Bibliotheca Mathematica*, $VIII_3$ (1907), p. 149. In regard to the first algoritm, however, ENESTROEM is mistaken in thinking that *articulus* there is equal to $A.10^n$, where A is any number, even greater than 10.

(106) *Mathematische Beitraege zum Kulturleben der Voelker*, p. 209; Geschichte der Mathematik, I^1, p. 583.

A digit is any number represented on the abacus by any apex on the column of the units. An articulus is a number represented by an apex on one of the following columns. All these numbers if represented by one apex only are also called simple or non-composite numbers. The composite numbers are such that are expressed by two or more apices.

17. *Various Modifications. The Scholastic Confusion.*—There occur, however, in the Latin sources various modifications of this widely spread and commonly accepted definition of the term *articuli*. So we find that in the Algorismus of SACROBOSCO (c. 1250) the tens are called *articuli principales*. He says : *Secundus vero (limes) novem articulorum principalium ; tertius centenariorum* (107). Another deviation is found in the *Demonstratio Jordani* (108), where the *articulus* is defined as *numerus denarius vel qui precise constat ex denariis. Item articulus est numerus qui potest dividi in decem partes equales*. The article is thus $A.10^n$, where A is any number, even greater than ten. This would include also some of the composite numbers that are represented by two apices, as f.i. $a.10^n + a.10^m$, which is obviously wrong. The same definition is also found in the oldest English Algorism (c. 1300), which quotes it from the *Carmen de Algorismo* of ALEXANDRE DE VILLA DEI (c. 1240), and in RECORDE's *Ground of Artes* (109).

A somewhat peculiar definition is to be found in some passages of the *Liber Algorismi de pratica arismetrice* (110). Here the number one is not counted to the digits, probably because it is not regarded as a number. Consequently the *articuli*, being the decimal multiples of the digits, must also start with 20, 200, 2000, etc. The numbers one, ten, hundred, etc. are called the limits, of which it is said (111) : *Illos autem ex quibus omnes isti* (112) *nascuntur, vocavit limites, quasi singularum generationum*

(107) Quoted by ENESTROEM, *Bibl. Math.*, XI_2 (1897), p. 98.

(108) Quoted by ENESTROEM, *ib.*, $VIII_3$ (1907), p. 149.

(109) See SMITH, *History of Mathematics*, II, p. 14-15.

(110) On pp. 131-35 and p. 26, lines 24-33. In the latter passage it is evidently a gloss that entered the text. Because in this part of the book, representing the adaptation of the *Algoritmi de numero Indorum*, another definition is prevailing; see § 16.

(111) P. 132, line 3 *seq.*

(112) The digits and articles.

primos parentes. A similar conception is represented by DE SACY regarding the Arabic sources (113).

There is small wonder, therefore, that some of the mediaeval mathematicians and of the modern historians were quite bewildered by all this confusion. Professor D. E. SMITH concludes his discussion of this terminology (114) with the words : « At best such a classification is unwieldy, and many of the more thoughtful writers, like FIBONACCI (1202), abandoned it entirely (115). Others, like SACROBOSCO (c. 1250), struggled with it but were obscure in their statements; while RAMUS (1555) very wisely dismissed the whole thing as puerile and fruitless, *puerilis et sine ullo fructu* ».

C. — THE HEBREW SOURCES

18. *Ibn Ezra (c. 1150) and the Talmud. Summary and Singular Numbers*.—In the preceding chapters we saw that there are three fundamental conceptions of the terms *articuli* and *'uqūd*, if taken as absolute values. They are : 1. The nine numerals, or apices of the abacus, in general; 2. the tens; and 3. all the higher orders, or the apices when standing in the columns following the units. IBN EZRA, in his *Sefer ha-Mispar*, (116) employs two terms for the two classes of numbers. The units are called *perāṭīm*, « singular, special » numbers, or also *aḥādīm*, units. All the higher orders are called *kelālīm*, « summary, general » numbers, and a distinction is made between the higher and lower *kelālīm*. SILBERBERG, the editor and translator of the *Sefer ha-Mispar*, renders the word by « Dekaden, Dekadenzahlen, » or also « Hauptzahlen, Komplexzahlen » (117).

IBN EZRA knew AL-KHŪWĀRIZMĪ's algebra (118), but certainly

(113) See § 12.

(114) *History of Mathematics*, II, pp. 12-15.

(115) Nor does the term *articulus* occur in the Algorismus edited by CURTZE and composed before 1168, nor in the *Algoritmi de numero Indorum*; see ENESTROEM, *Bibl. Math.*, VIII$_3$ (1907), p. 149.

(116) Ed. SILBERBERG, Frankfurt a/M, 1895, pp. 1, 2, 4, etc.

(117) Cf. p. 92, note 12. Similarly, ROSEN, tranlates *'uqûd* either with « complex numbers » or with « greater numbers »; see § 12.

(118) Cf. the writer's « Origin of the term root, second article » in *The American Mathematical Monthly*, vol. 35 (1928), p. 71, note 2.

had no knowledge of the Latin sources. Yet, he must have understood the *'uqūd* as including all the higher orders. As a purist of the Hebrew language, IBN EZRA does not give a literal translation of the term *'uqūd* but employs the terminology known already from the Talmudic literature (119). *Kelal* and *peraṭ* are used there as logical terms for general and special conceptions, then also *kelal* as the sum total in the addition of several numbers, and finally as the term for the tens and hundreds in opposition to the units that are called *perāṭīm*.

It is interesting to note that in the Talmud the two classes *kelal* and *perāṭ* do not reflect the strict distinction between units and higher orders peculiar to the Hindu system. In the same place (120) we find also the *kelalīm* for the hundreds, the *peraṭā rabbā*, « the great singular » for the tens, and *perāṭā zuṭṭā*, « the small singular » for the units. IBN EZRA, however, being acquainted with the Hindu system, adapted the talmudical terminology to the Arabic *aḥād* and *'uqūd*. The *perāṭīm* are used for the uints only and the *kelālīm* are distinguished as « higher » and « lower ones » (121). It seems, however, that the same talmudical classification into « summary » and « singular » numbers was already known to the ancient Romans and preceded the terminology of the digits and articles (122).

IV. — CONCLUSION.

19. *Two Conjectures.*—In concluding this investigation of the *'uqūd* and *articuli*, we see firstly that the testimony of MAIMONIDES is confirmed. 1. The *'uqūd* as well as the *articuli* originally referred to the numerals in general. 2. The term *articuli* in this meaning is found in the works of ALCUIN of York, hence is an original Roman term.

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(119) *Bekhorot*, 5a; see also *Baba Batra*, 123a, and the talmudic dictionaries.

(120) *Bekhorot*, 5a.

(121) The same distinction between higher and lower *kelâlîm* is also made in other, non-mathematical, classification of the mediaeval Hebrew; cf. the passages quoted in BEN IEHUDA'S *Thesaurus*, p. 2402.

(122) See § 16, note 99. The term *singularis* for the units is also used on the abacus of BERNELINUS OF PARIS (c. 1020); see CANTOR, I[4], p. 881. The Greeks called the units *πνθμένες*. The tens were called by ARCHIMEDES *οἱ ἀνάλογοι ἀριθμοί (numeri pertinentes)*; see TROPFKE, I[2], p. 11.

But why did they call the numerical signs by the name *articuli*, « knots » or « joints »? The answer to this question can be given only in a vague conjectural way. After all, we also do not know exactly why the alphabetical signs, the letters, were called *litterae*. Some people hold (123) that *litera* comes from *linere*, *litum*, « to besmear, to spread over », because one of the earliest modes of writing was by graving the characters upon tablets smeared over with wax. Others (124) say that the origin of the Latin word is obscure and that it probably has no connection with the root of *linere*. The word « sign », that we use, comes from the Latin *signum*, meaning « flag, standard, banner », or, what is more likely, from the meaning « to seal a stamp ». The knots were also signs and seals and symbols (125). And perhaps they were called joints because in the fingerreckoning the joints represented all the numerals. The phenomenon of the numerals was there and people were looking for a fitting name. At the time of their coinage, the names were more or less fitting. Nowadays, after some 1000 or 2000 years, it is not always easy to find out their original meaning. Thus some people called the numerals *caracteres* (126), a term used for the letters and meaning « an instrument for marking », Greek χαρακτήρ. Others, in order to differentiate, called them *apices* and *metae* (127), meaning « crown, cone, conical hat, top, the least particles and tittles of letters » (128). In the talmudical Hebrew, too, these small tittles and points of the letters are sometimes called *qōṣ*, *uqqāṣ*, « thorn, point, sting, tail », and sometimes *keter*, *tāgā*, « crown ». A legend relates that the Lord was sitting and « tying the crowns » around the letters (129). If these crowns were tied to the letters, then they had also some connection with knots. The Arabs called the numerals and letters *ṣūrāt*, « figures », and also *raqm*, meaning originally « a striped stuff, a variegated,

(123) See the WEBSTER dictionary.
(124) See *Encyclopaedia Britannica*, 11th edition, vol. 16, p. 501.
(125) See « The Knot in Hebrew Literature », *Isis*, vol. 14, p. 211, note 89.
(126) See the BOETHIUS geometry, p. 397, lines 3, 18; p. 426, line 24.
(127) See § 5.
(128) Cf. *iota out apex*.
(129) Cf. *Shabbat*, 104b, *Erūbin*, 13a; *Shabbat*, 89a, *Menaḥot*, 29b.

embroidered stuff » (130). The Hindu numerals, of course, were called *raqm hindiyy*, « Hindu embroidering ».

Yet, the term *articuli* applied only to the abacus numerals, known to us as ghubār numerals. The ancient Roman numerals (I, II, III, etc.) apparently based on the practice of holding up the fingers of the hand were probably called *digiti*. When people were looking for a new terminology applying to the relative units and tens resulting from the multiplication on the abacus, they named the units *digiti*, after the ancient numerals used for the units only, and the tens, *articuli*, after the new numerals used for all the orders (131).

20. *Summary*.—The writer hopes that the present investigation into the terminology : 1. ghubār-pulvis-abacus, and 2. *'uqūd-articuli*, might prove helpful not only to students of history of mathematics, but also to philologists interested in the texts, where these terms occur (132). The critical method employed in this paper might perhaps give a clue for the determination of their time, or of the school to which they belong. In a sense, such terms, rightly dismissed by the mathematicians as « puerile and fruitless » (133) may be likened to the fossil vestiges by which the geological layers and strata are determined. In a sense, there is still plenty of « philological rubbish » to be excavated and deciphered as historical records.

Briefly summarized, the present study led to the following conclusions : I. The writer's opinion concerning the Arabian abacus is confirmed by new facts : 1. That ghubār is the abacus, is testified to by Rabbi JUDA BARṢELONĪ. 2. The existence of a corresponding Latin term, *pulvis* and *pulvisculus*, is proved. II. New evidence is adduced in proof of MAIMONIDES' testimony : 1. The *'uqūd* are the terms for the ghubār numerals. 2. They go back to the Latin *articuli*, occurring already in ALCUIN's works. III. All this goes to confirm the old theory of WOEPCKE as to the Roman origin of the ghubār numerals. Even as we today speak

(130) Cf. the Hebrew *riqmah*.

(131) See § 14.

(132) Especially the terms *'uqûd-articuli*. The *Thesaurus linguae Latinae*, under the item *articuli*, 2, *col.* 696, No. 5, cites only the definition of the BOETHIUS geometry, without any further comment.

(133) See § 17.

of Arabic and Roman numerals, indicating their origin, so did the Arabs have two types of numerals, Hindu and Roman, whose origin was indicated by the names Hindu and ghubār, and so did the Romans have two types of numerals, which were called *digiti* and *articuli*.

Thus we have now three types or numerals : 1. The Hindu numerals accepted and still retained by the Eastern Arabs. 2. The ghubār numerals. The Western Arabs learned them from the Roman abacus and called them the ghubār numerals, or *'uqūd*. The Europeans learned them from the Arabs, and called them the Arabic numerals. The Romans called them *articuli*, *apices* and *metae*. 3. The ancient Roman numerals, that were called *digiti*.

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Zur Geschichte der Zuordnung von Stoff und Eigenschaft [1]

Das neunzehnte Jahrhundert ist die grosse Zeit der *reinen* Chemie. Rasch wächst die Zahl der neuaufgefundenen Elemente, gewaltig die der in der Natur aufgefundenen und der kunstvoll im Laboratorium zustande gebrachten Verbindungen, und nach immer erneuten Anläufen nähert man sich der Erkenntnis eines systematischen Zusammenhangs der neuen Erfahrungen. Ein sehr einfaches Anordnungsprinzip für die *Elemente* lässt unerschöpflich neue Zusammenhänge auffinden, zwischen den *Verbindungen* stellen die experimentellen Methoden und manche allgemeine Regeln Beziehungen her. Die Herstellung *reiner* Stoffe ist das grosse Ziel; man sucht nach Methoden, um einheitliche Stoffe aus ihren Mischungen mit nahezu gleichartigen abzutrennen. Was in der *Natur* an Stoffen untersucht wird, erweist sich nun als Gemisch verschiedener kompliziert aufgebauter Gebilde, von denen meist mehrere Verwandte gemeinsam auftreten. Die grossen Untersuchungen von CHEVREUL über die in natürlichen Fetten vorhandenen Fettsäuren leiten das noch junge Jahrhundert in mancher Hinsicht verheissungsvoll für die Chemie ein. Die *künstlich* veranlassten chemischen Umbildungen nimmt man ebenfalls nicht mehr als einheitlich hin, sondern erkennt sie als Gemische, mindestens als noch « verunreinigt » durch auszuscheidende Nebenprodukte. Aus solchen in Reinheit isolierten *Stoffen* soll das System der Chemie aufgebaut werden.

Ehe sich dies noch ganz entfalten konnte, wurde allerdings schon ein Vorstoss in die Richtung *physikalischer* Betrachtungsweise versucht. Dies geschah durch die Substitutionstheorie in der organischen Chemie; man hat damit « den Schluss ziehen

(1) Nach einem Vortrag auf der Hauptversammlung des Vereins Deutscher Chemiker, Frankfurt, 11. Juni 1930.

wollen, dass der Charakter einer chemischen Verbindung weniger von der Natur als der Lagerung der sie constituierenden Elemente abhängig ist. Allein gerade das Anilin mit seinen Substitutionsprodukten widerspricht dieser Ansicht aufs Entschiedenste... » (2) Diese Worte aus der Mitte des vorigen Jahrhunderts kennzeichnen den Kampf der chemischen Betrachtungsweise, nach welcher der Stoff die Einheit bildet, gegen die wiederauflebende physikalische, in Eigenschaften als Einheiten zerlegende.

Die chemische Auffassung siegte. Nicht die einzelne Eigenschaft stellt die letzte chemische Einheit dar, sondern der eigentümliche — das Fremdwort individuelle sagt es vielleicht noch besser — Komplex von Eigenschaften, den der Stoff bildet. Auf diesem Grunde ruhte auch die Ueberwindung der Phlogistik durch die Sauerstofftheorie; denn die Phlogistik war ein Ausdruck für die Verselbständigung der Eigenschaften, und das Phlogiston war ein Eigenschaftsstoff. Aber nicht Eigenschaften, sondern die Eigenschaftsvielheiten der Elemente sind letzte Erklärungsgründe in der reinen Chemie.

In unserem zwanzigsten Jahrhundert ist eine andere Phase der Materieforschung stärker hervorgetreten. Sie ist nicht im Grunde, sondern in der Ausbildung, in der erfolgreichen Ausdehnung der physikalischen Forschungsmethoden und ihrer mathematischen Erfassung neu. Die eigenschaftslose Materie und der materielle Eigenschaftsstoff hatten nach langen Auseinandersetzungen dem materiell einheitlichen Eigenschaftskomplex Stoff weichen müssen. Jetzt tritt die Aufgabe wieder in den Vordergrund, die materiellen Bedingungen für die einzelnen Eigenschaften zu erkunden. Bei den mancherlei Schwierigkeiten, die dieses Unternehmen mit sich bringt, mag ein zusammenfassender Rückblick auf die Vorstellung vom Zusammenhang zwischen Stoff und Eigenschaften berechtigt sein; denn wenn mancher in der Gegenwartsarbeit stehende Forscher vergangenen Meinungen und Zuständen bestenfalls Raritäts-Charakter zuerkennen möchte, so zeigt genaueres Eingehen doch, dass Nachwirkungen auch heute noch vorhanden sind : einmal in der Form, dass die früheren Denkweisen den Grund der neueren

(2) *Handwörterbuch der reinen und angewandten Chemie*, hg. von LIEBIG u. A. Bd. 3, S. 441 (1848).

Wissenschaftsgebäude bilden, zum anderen auch darin, dass sie Wege zeigen, an die wir von jeher gebunden waren und auch jetzt noch bleiben.

Aus so etwas wie der allgemeinen und ihrem Wesen nach monistisch einheitlichen Materie soll ihre Besonderung zu den Stoffen abgeleitet werden. Merkwürdigerweise geschieht dies zum Teil dadurch, dass der Materie zunächst einmal die materielle Natur abgesprochen wird. Dies ist ein sonderbares Kennzeichen unserer als materialistisch verschrieenen Zeit : sie führt die Anschauung zum Siege auch bei den extakten Naturwissenschaftlern, die die « grobe » Materie zu elektrischen Spannungen und Kraftfeldern vergeistigt. Im Ueberschwange des Stolzes auf die neue grosse Leistung kamen jetzt solche Aussprüche zustande, wie man sie bei H. WEYL findet : « *Ich bin fest davon überzeugt, dass die Substanz heute ihre Rolle in der Physik ausgespielt hat...* Die Physik muss sich ebenso der *ausgedehnten* Substanzen entledigen, wie die Psychologie schon längst aufgehört hat, die Gegebenheiten des Bewusstseins als « Modifikationen » aufzufassen, die einer einheitlichen Seelensubstanz inhärieren » (3).

Uns Chemikern bleibt auch gegenüber den Anschauungen moderner Physiker die Realität der Stoffe und ihrer Verschiedenheiten recht eindringlich klar. Aber die Erfolge der einzelnen neuen Auffassungen bringen auch der organischen und anorganischen Chemie neues Leben. Dem entspricht die praktische Auswirkung auf die technische Chemie. Wenn man heute die Schmierwirkung eines Oeles erklären will, dann bezieht man sich auf den kammartigen Bau seines Moleküls und arbeitet mit so anschaulichen mechanischen Vorstellungen wie dem durch die Konstitution ermöglichten Ineinandergreifen und Verhaken der Moleküle, die so den Schmierfilm aufbauen (4).

Das ist eine andere Denkmethode als die bisherige. Wenn man die Beziehungen zwischen Lichtabsorption und chemischer Konstitution zu erfassen suchte, so begnügte man sich damit, gewissen Atomgruppierungen, etwa der Azo-Gruppe, besondere Absorptionsbänder zuzuordnen. Die Erklärung führte dabei auf

(3) *Die Naturwissenschaften*, 1924, S. 568.

(4) WOOG, *Contrib. à l'étude du graissage*, Paris, 1926, vgl. *Zeitschr. Vereins Dtsch. Ing.* 74, S. 371 (1930).

die *Qualität* der Atome oder Atomgruppen zurück, während neuerdings die *Form* dafür eintritt. Die auf Qualitäten sich gründende Erklärung ist eine rein statistische : bei soundsoviel Verbindungen mit dieser Atomgruppe erscheint dieses bestimmte Absorptionsgebiet. Die physikalische Zurückführung auf die Form geht viel tiefer auf Kausalgründe, indem sie einen einfachen Mechanismus heranzieht.

Diese neuen Töne in der Chemie klingen dem vertraut, der von ihrer Geschichte etwas weiss. Hat nicht schon BOYLE die ganze Chemie auf die Betrachtung von « Materie und Form » aufbauen wollen und war dabei noch nicht Neutöner, sondern Nachfolger jener Gedankenrichtung, die bis auf DEMOKRIT und die antike *Atomistik* zurückgeht ? Gab es nicht einmal im Mittelalter der Chemie eine Zeit, wo man genau wusste, dass die schweflige Säure ihre Eigenschaften, wie Flüchtigkeit und stechenden Geruch, der Form ihrer wie feingeschliffene Messerchen gestalteten Atome verdankte ? Wir lächeln, wenn wir solche Erklärungsversuche begeistert vorgetragen finden — solange sie von LEMERY oder gar von LUKREZ vorgetragen werden. Es ist anzunehmen, dass es diesen alten Denkern mit ihren Theorien recht ernst war, und die Erinnerung daran soll keineswegs die neue Gestalt der physikalischen Eigenschaftserklärung nach der Unvollkommenheit ihrer früheren Ausbildungsstufen kritisieren. Die Arbeit an der Geschichte der Chemie besteht darin, dass sie die Zeitkoordinate nicht vergisst, sondern betont. Wohl liegt die Extrapolation nahe, wie nach einem weiteren Entwicklungsablauf unsere heutige chemische Physik ähnlich beurteilt werden könnte, wie zu unserer Zeit LEMERYS Spekulationen, aber so werden nur solche Kritiker schliessen, die aus der Geschichte nur die Vortrefflichkeit ihrer eigenen Zeit zu ersehen vermögen.

Mit der auf die Antike zurückgehenden Konstruktion einer qualitätslosen Materie hängt die Entwicklung zu der Erkenntniskritik zusammen, die schliesslich alle Eigenschaften als blosses Spiel der Einbildung, als subjektive und darum falsche Zutat brandmarkte und aus der Wissenschaft ausschliessen wollte. Man wird auch die entsprechenden Folgerung aus den modernsten Anschauungen gezogen finden. Der Auffassung von der qualitätslosen Materie steht heute noch die andre gegenüber, wonach die stofflichen Elemente als die qualitativen Grundwesen gelten.

Das war bei den alten « Philosophen » nicht der Fall; denn die Denkweise des Aristoteles bedeutet eine Fundierung auf Eigenschaften als letzten Zerlegungseinheiten. Dass diese wieder je durch einen Stoff vertreten werden, bedeutete nur die Veranschaulichung eines Symbols. Es gibt nach jener klassischen Hypothese nicht elementare *Stoffe*, aber es gibt elementare *Eigenschaften*. Die wirklichen Stoffe zeigen mehrere Eigenschaften zugleich, darum sind sie aus diesen Eigenschaften zusammengesetzt. Die Eigenschaften sind ihre stofflichen Bestandteile, und der wirkliche Stoff ist gleich ihrer Summe. Erst in langer Entwicklung und immer wieder versuchter Anpassung an neue Erkenntnisse gelangte man zu der Einsicht, dass man nicht Eigenschaften isoliert darstellen kann, sondern immer nur Stoffe.

Wir erkennen an einzelnen Eigenschaften die ganzen Stoffe, und oft kennen wir die Eigenschaften vor den Stoffen, zu denen sie gehören. Als SCHÖNBEIN den Ozongeruch entdeckte (1839), blieb er lange Zeit allein mit der Schlussfolgerung, dass damit ein neuer Stoff Ozon angezeigt würde. Als BUNSEN und KIRCHHOFF (1861) an Salzen des Dürkheimer Mineralwassers neue Spektrallinien auffanden, hatten sie es leichter, die Fachwelt davon zu überzeugen, dass ein neues Element darin vorhanden sein musste. Bei der Erforschung der radioaktiven Erscheinungen ging man davon aus, Vorhandensein und Beständigkeit von Stoffen aus Strahlungskurven abzuleiten, ähnlich wie man aus genau berechneten Bahnstörungen die Existenz neuer Planeten vor ihrer « Entdeckung » beweisen konnte.

Worin bestand der Beweis? Es war nicht immer möglich, wie beim Cäsium, das vorausgesagte neue Element als Stoffteilchen auf die Wage zu legen. Aber auch die schärfsten Ansprüche waren erfüllt, wenn an der Stelle, wo die eine neue Eigenschaft beobachtet wurde, das Auftreten gewisser anderer Eigenschaften belegt werden konnte. Da sehen wir nicht Stoffe, sondern Wirkungen. Wir sehen Enzyme auch heute noch nicht als stoffliche isolierte Dinge, und doch zeigt die Verknüpfung der eigenartigen Enzymeigenschaften mit andern physikalischen Verhaltungsweisen, wie Adsorbierbarkeit und Trennbarkeit von allen sonstigen bekannten Stoffen, dass die Enzyme neue besondere Stoffe sind.

Dieses bis jetzt stets bestätigte gemeinsame Auftreten verschiedener Eigenschaften ist das Ergebnis der jahrhundertelangen

Arbeit an der Ueberwindung des Aristotelismus in der Chemie. Nicht die Eigenschaften sind Elemente; denn eine Eigenschaft tritt immer zugleich mit anderen auf, ist nicht getrennt beständig und darum kein Element. Vom Phlogiston hatte man sich durch ein solches Argument befreit. Aber auch danach noch behielt die Chemie eine gewichtige Anzahl von Eigenschaftsstoffen bei. Schon dass der Sauerstoff immer der Säuremacher sein sollte, hat er vom Phlogiston als seinem Gegensatze übernommen (5). Alle organischen Säuren galten als *Essigsäure*, nicht weil man die Unterschiede gegen sie nicht sah, sondern weil die Essigsäure als Eigenschaftsstoff galt. Das blieb bis in die Mitte des neunzehnten Jahrhunderts so, und noch im Jahre 1860 muss LIEBIG erst Beweise dafür erbringen, « dass die Weinsäure keine Essigsäure fertig gebildet... enthält » (6). Hier befand sich die organische Chemie im Jahre 1860 in einem Zustande, wie ihn 1660 ROBERT BOYLE für die Chemie der Elemente angetroffen hatte. Sein Nachweis, dass ein Körper schwer sein kann, ohne das Element Erde zu enthalten, entspricht dem, was LIEBIG für die organischen Säuren zu beweisen suchte. — So gab es lange Zeit und bis ins neunzehnte Jahrhundert hinein auch einen und zwar nur einen *Riechstoff*, der allen bekannten Duftstoffen innewohnte und sie zu Duftstoffen machte.

Wenn Eigenschaften subjektiv bedingt sind, so haben wir sie nach langer Arbeit objektiv feststellbar gemacht. Aber weil sie subjektiv sind, bedeuten sie für das alltägliche Verhalten mehr als die Stoffe. Daraus entsprang die alchemistische Praxis, die mit Färbungen von Steinen und Metallen begann. Von den mannigfaltigen Eigenschaften der Stoffe haften nur die oberflächlichst wahrnehmbaren im Gedächtnis des Publikums, darum gibt es gerade diesen Eigenschaften die Bedeutung von Wesenseigenschaften. Das *Färben* der Stoffe gewinnt daher besonderen, aber vieldeutigen Sinn. Mit einer neuen Farbe nimmt der Stoff zugleich einen neuen Charakter an. Die Alchemie vermag also neue Stoffe herzustellen, wenn sie nur neue Färbungen hervorruft. Dass aber nicht alles Gold ist, was wie Gold aussieht, muss für

(5) Vgl. dazu FÄRBER in *Studien zur Geschichte der Chemie* (Lippmann-Festschrift), S. 122 ff. Berlin 1927.

(6) *Liebigs Annalen* 113, S. 11 (1860).

die wahrhaften Jünger der Goldmacherkunst eine späte und erschütternde Erkenntnis gewesen sein. Uebereinstimmung in einer Eigenschaft beweist noch nicht die Gleichheit zweier Stoffe. Bei ZOSIMOS (um 300) heisst es : « Das färbende Prinzip ist es, welches dem Dampf die Fähigkeit gibt, Gold zu entwickeln, gewöhnliches Gold hat nur für sich selbst genügend Gelb » (7). Hier bestimmt die Qualität : Farbe die Erkennung und Bewertung des ganzen Stoffes. Mehr als ein halbes Jahrtausend später erst vernimmt man beginnende Kritik : In DSCHABIR's « Buch des Mitleids » heisst es : « Das Aussehen der Oberfläche und des Aeusseren täuscht eben über das Innere » (8). Im 17. Jahrhundert — Entwicklungen geschehen langsam in dieser Zeit — ist die Umkehrung vollständig geworden : Nun ist Farbe nur eine zufällige Eigenschaft, abhängig von Ausbildungen der Oberflächen, die fast beliebig beeinflusst werden können, ohne dass der Stoff selbst sich ändert. Diese von BOYLE aus den Anlauffarben von Stahl abgeleitete Auffassung herrschte auch im 18. Jahrhundert vor. Sie kehrt in dem « Chymischen Wörterbuch » von MACQUER in folgender Weise wieder : « Es geschieht demnach vorzüglich durch das Anlegen der Farbeteilchen der färbenden Materie an die Oberflächen, wo sie (nämlich die Oberflächen vorher) nichts deckt, dass sie den zu färbenden Materien Farbe beibringen. Es ist eine Art Malerey, weil die Teilchen sich vermittels des Berührens und kraft der grösseren oder geringeren Verwandtschaft, die sie zufolge ihrer Natur mit den Teilen der Oberfläche haben, an solche anlegen oder anhängen » (9). « Einige suchen die materielle Ursache der Farben oder der färbenden Teile bloss in dem Brennbaren; andere hingegen in dem Eisen ». Nach MACQUER scheint sie nicht in einem eigentlichen *Farbestoffe*, « sondern in dem Zustande einer jeden Art Materie zu liegen » (10). Diese Geringschätzung der Eigenschaft : Farbe wirkt noch auf SCHOPENHAUER und veranlasst ihn, mit der an idealistischen Philosophen kennzeichnenden Absolutheit, zu behaupten, dass die Farbe von jeher als eine sekundäre Qualität galt und dass

(7) Vgl. HOPKINS in *Studien zur Geschichte der Cheme*, S. 9 ff., Berlin 1927.
(8) BERTHELOT, *La Chimie au Moyen-Age*, Bd. 3, S. 178; LIPPMANN, *Entst. u. Ausbr. d. Alchemie*, S. 366 (Berlin 1919).
(9) Band 2, S. 419 (1788).
(10) *Chymisches Wörterbuch*, Bd. 2, S. 407 (1788).

« überhaupt kein Philosoph jemals die Farbe für einen wirklich wesentlichen Bestandteil der Körper hat wollen gelten lassen, während mancher nicht etwa nur Ausdehnung und Gewicht, sondern auch jede Beschaffenheit der Oberfläche, das Weiche und Harte, Glatte und Rauhe, ja zur Not lieber den Geruch und Geschmack des Dinges für wirkliche konstituierende Bestandteile (!) desselben gelten liess, als die Farbe » (11).

So wandelte sich die Farbe in der Auffassung der Chemiker und der Theoretiker von einer *Wesens-Eigenschaft* zu einer *Bedingungs-Eigenschaft*. Aber ein Teil dieser Entwicklung führt weiter und in die Nähe des Ausgangspunktes zurück. BERTHOLLET deutete für Indigo und seine Verküpbarkeit eine chemische Theorie an (12). Das fand zunächst noch keine Nachfolge. PREISSER machte einen gleich zu kühn ausgedehnten Versuch, alle organischen Farbstoffe als Oxydationsstufen von ursprünglich farblosen Verbindungen zu erweisen (13). Der Nachweis, dass das nicht so allgemein zutraf (14), scheint die Weiterarbeit auf diesem Gebiete zunächst abgeschreckt zu haben. So musste man sich um die Mitte des 19. Jahrhunderts damit begnügen, festzustellen : « Die Farbstoffe bilden keine eigene Gattung von bestimmtem Charakter, wie z.B. die Fette, die Zuckerarten u.A. » (15). Aber wenige Jahre danach wurde der erste künstliche organische Farbstoff hergestellt und durch A. W. VON HOFFMANN eine neue Entwicklung auf diesem Gebiete daran geknüpft. Die erste allgemeinere Theorie liess nun nicht mehr lange auf sich warten; es ist diejenige von GRAEBE und LIEBERMANN (16), die noch einige ähnliche Bestimmungsstücke wie bei PREISSER enthält, aber in ihrer Beschränkung auf einzelne Fälle viel tiefer dringen konnte. O. N. WITT's Theorie (17) führt um den hier entscheidenden Schritt weiter : « Die Farbstoffnatur aromatischer Körper ist bedingt durch die gleichzeitige Anwesenheit einer Farbstoff

(11) *Werke*, Bd. 6, (Reclam-Ausgabe). S. 56.

(12) *Éléments de l'Art de la Teinture*, (1805), vgl. FÄRBER in *Das Buch der grossen Chemiker*, Bd. 1, S. 346, Berlin 1930.

(13) *Journ. pharm. chim.*, 3 Serie, Bd. 5.

(14) Vgl. z. B. *Liebigs Annalen*, Bd. 62, S. 129.

(15) *Handwörterbuch der reinen u. angew. Chemie*, hg. von LIEBIG u. A. Bd. 3, S. 4 (1848).

(16) *Ber. d. dtsch. Chem. Ges.*, Bd. 1, S. 106 (1868).

(17) *Ebenda*, Bd. 9, S. 522 (1876).

gebenden und einer Salz bildenden Gruppe ». Das Zusammenwirken von Chromogen und Auxochrom (1888), nämlich von zwei bestimmten Gruppierungsweisen eines *Teiles* der Atome im Molekül, erteilt diesem selbst die Farbstoffeigenschaft. Ein stofflicher Teil des Stoffes bringt eine seiner kennzeichnenden Eigenschaften hervor. Aber es ist keine elementare Farbstoff-Materie, die dies tut. Vielmehr ist jener Teil nicht als solcher isolierbar; man erkennt ihn dennoch in vielen Einzelheiten, weil man ihn an andere ebenfalls nicht für sich beständige Stoffteile versetzen kann.

Doch ist die Nichtisolierbarkeit kein definierendes Kennzeichen für diese eigenschaftstragenden Stoffteile. Sie sind « *Radikale* », und über die mögliche stoffliche Selbständigkeit der Radikale hat man seit hundert Jahren immer wieder mit Argumenten und Experimenten diskutiert (18). Diese Frage geht nicht nur die Valenzlehre an; sie erhält besonderes Gewicht durch den Untergrund von alten Vorstellungen über die selbständigen Eigenschaftsstoffe. Wenn man Radikale vom Charakter farbgebender Gruppen isolieren könnte, dann hätte man die Richtigkeit der Existenz der alten Eigenschaftsstoffe erwiesen ! Bei allen Unterschieden zwischen « elementarem Salz » oder « Schwefel » und solchen Radikalen bliebe doch die Gemeinsamkeit des gedanklichen Aufbaus nicht zu verkennen.

Aber wenn sich die Vorstellung von bestimmten, Farbstoff-Eigenschaften verleihenden Radikalen den Vorstellungen von Eigenschafts-Elementen in der allgemeinen Denkhaltung zu nähern schien, so erkennt man neuerdings, dass dies ein vorläufiger und meist vergröbernder Behelf war. Immer stärker beachtet man die feineren Einflüsse, die von allen Teilen des Moleküls her wirken, um zum Verhalten des Stoffes zusammenzuklingen.

Das Verfahren, Eigenschaften tragende Atomgruppen zur Erklärung heranzuziehen, erwies sich auch in vielen anderen Fällen als für erste Annäherungen geeignet. Besonders erfolgreich waren die Deutungen dann, wenn sie auf zwei zusammenwirkende « qualiphore » Gruppen ausgingen; man hat sogar die Harzbildungs-Fähigkeit mit der Anwesenheit von resinophoren und resinogenen Molekülteilen zu erklären versucht.

(18) Vgl. *die Darstellung der Entwicklung des Radikalbegriffs* bei WALDEN, *Chemie der freien Radikale*, Leipzig, 1924.

Aehnlich ist die Entwicklung in der Erkenntnis der chemotherapeutischen Eigenschaften gegangen. Die Wirkungen der *Arzneimittel* erklärte man in alter Zeit mit ihrer ganzen « Natur » oder mit ihren dem kranken Körper fehlenden « Elementen ». Besonders einfach machten es die Heilpflanzen den Menschen; denn sie zeigten durch die Form ihrer Wurzeln, Blätter oder Blüten, welche Art von « Kräften » in ihnen steckten. Bei PARACELSUS ist Chelidonium ein Mittel gegen Gelbsucht, weil seine Blüten gelb sind. Wenn eine Pflanze nierenförmige Blätter hat, so äussert sie damit ihr Wesen, Nierenkrankheiten zu heilen. Da der Schafbock « hitzig » ist, muss sein Blut die Eigenschaft haben, selbst Diamanten zu erweichen (19).

Welch primitive Art von Analogienbildung ! Wie weit stehen diese vermeintlichen Erkenntnisse zurück hinter der Naturverbundenheit, durch die ein südamerikanisches Naturvolk die Eigenschaften der Chinarinde fand ! Nur wenn es eine Theorie dazu hätte aufstellen sollen, wäre sie wahrscheinlich nicht weniger primitiv ausgefallen.

Dass man aus Pflanzenteilen die wirksamen Prinzipien, wohl nach dem Vorgange von RAYMUNDUS LULLUS (20), in der Form von Tinkturen und Essenzen ausziehen konnte, stimmte gut zu der Anschauung von Eigenschaftsstoffen, welche auf diese Weise — nahezu — isoliert wurden. Seit einigen Jahrzehnten sucht man an den wirklich in möglichster Reinheit isolierten Stoffen ihre therapeutischen Wirkungen auf die Anwesenheit von gewissen Substituenten oder Seitenketten zurückzuführen. Bald glaubte man, die Synthese von Arzneistoffen aus solchen Eigenschaftsträgern vorausberechnen zu können. PAUL EHRLICH's Hypothesen führten eine Glanzzeit der Chemotherapie herauf und brachten grosse Erfolge, die dennoch nicht vollständig zum Beweise der Theorie ausreichten. Je tiefer die Forschung drang, um so stärker traten Ausnahmen hervor. Wie soll man auch allgemeine Regeln finden, wenn der Ersatz einer Methylgruppe durch die nächstverwandte Aethylgruppe in einem fast hundert Atome enthaltenden Molekül die therapeutische Wirkung nicht nur dem Grade nach verändert, sondern vollständig entscheidend beeinflusst ? (21)

(19) E. O. VON LIPPMANN, *Chem. Zeitung*, 1921, S. 2; 42, 62.
(20) Vgl. sein Buch : *De secretis naturae*, um 1300.
(21) Vgl. J. MORGENROTH, *Naturwiss.*, 12, S. 219, 224 (1924).

Viel einfacher liegen die Verhältnisse bei der *Refraktion*. Die Brechungsexponenten stehen in enger Abhängigkeit von der chemischen Natur der Stoffe. Jedes in einer Verbindung anwesende Element trägt in bestimmter und wie es zuerst BERTHELOT (1856) und den Begründern der Refraktometrie organischer Stoffe (22) schien, in *konstanter* Weise zu dem Werte des Brechungsvermögens bei. Allmählich musste dieses einfachste Gesetz modifiziert werden. Das Brechungsäquivalent des Sauerstoffs hängt davon ab, in welcher Art von Bindung es vorliegt. So ging es in einer immer grösseren Zahl von Fällen, bis daraus erkannt wurde, dass das Brechungsvermögen nicht eine additive, sondern eine konstitutive Eigenschaft darstellt. Nun (23) kann man sehr feine Verschiedenheiten im atomaren Aufbau von Verbindungen an den Brechungsvermögen erkennen.

Die Erforschung noch mancher anderer Eigenschaftsgrössen entwickelte sich ähnlich. Sie erschienen zuerst empirisch als so etwas wie zufällige *individuelle* Eigenschaften, dann als *additive* und schliesslich als *konstitutive*, wobei zuletzt immer wieder der *individuelle* Eigenwert an besonderen Feinheiten zur Geltung gelangt. Die elementaren Bestandteile bringen zwar ihre Eigenschaften mit in die Verbindung ein, die andern Teile der Verbindung verändern sie aber in charakteristischer Weise. Dies ist ein weiterer wichtiger Unterschied gegen frühere Vorstellungen. In den alten Elementen-Lehren wirken die Bestandteile nur additiv zusammen; daher stammt auch die Vorstellung, die im 18. Jahrhundert weitgehend herrschte : dass die Verbindung in ihren Eigenschaften das Mittel zwischen denjenigen der Komponenten bildet.

Nur eine Eigenschaft bleibt von Verbindungsarten- und Formen unbeeinflusst und streng additiv : das *Atomgewicht*, bis auf den Fall jener ins Innere des Atomkerns eindringenden Verbindung, die aus leichten Elementen schwerere entstehen lässt. Die Eigenschaften der Elemente selbst sind besonders merkwürdige Funktionen ihrer durch das Atomgewicht ausgedrückten stofflichen Natur. So konnte LOTHAR MEYER schon vor sechzig Jahren

(22) GLADSTONE und DALE, LANDOLDT, BRÜHL; Literatur siehe bei SMILES-HERZOG, *Physikalische Eigenschaften und chem. Konstitution*, 1914.

(23) Vgl. *die Untersuchungen* v. von AUWERS, EISENLOHR u. A.

den von verschiedenen Forschern nahezu gleichzeitig gehobenen Erfahrungsschatz durch die Sätze kennzeichnen : Die Erfahrungen mit den periodischen Anordnungen der Elemente nach den Atomgewichten und der verschiedenen Eigenschaften derselben ist ein Anfang zu dem Nachweise, « dass der Zahlenwert des Atomgewichtes die Variable ist, durch welche die substanzielle Natur und die von ihr abhängigen Eigenschaften bestimmt werden. Das Atomgewicht wird also in Zukunft als neue Variable in die Rechnung einzuführen, die Eigenschaften der Stoffe, die physikalischen Erscheinungen werden nicht nur als Funktionen des Ortes, der Zeit usw., sondern auch als Funktionen des Atomgewichts zu behandeln sein » (24).

Die *theoretischen* Untersuchungen über Eigenschaften haben von jeher ihre *praktischen* Gegenstücke gehabt. Das Problem der Ersatzstoffe ist schon früh lebendig gewesen, nämlich die Ersetzbarkeit eines Stoffes durch einen anderen, wobei die gewünschte Eigenschaft erhalten blieb, obwohl ein anderer Stoff als ihr Träger geboten wird. THEOPHRAST berichtet von künstlichem Lasurstein, der in Aegypten hergestellt wurde, in noch besserer Qualität aber in Babylonien zu haben war (25). Heute kann man viele *Edelsteine* so vollkommen « nachmachen », dass man höchstens unter der Quecksilberlampe Unterschiede gegen das Naturprodukt an der Ultraviolettfluoreszenz feststellen kann. Trotzdem haftet den künstlichen Edelsteinen ein Minderwert an. Geschieht das etwa, weil wir Edelsteine mit jener bestimmten Art von Ultraviolettfluoreszenz zu haben wünschen ? Nicht nach dieser sorgfältig im Laboratorium erkundeten Eigenschaft wird gewertet, hier wird die Entstehungsgeschichte zur wertbestimmenden Eigenschaft.

Gegen zahlreiche, heute sonderbar erscheinende Widerstände entwickelte sich die Industrie der « *künstlichen* » *Soda*. AUDOUAR schreibt darüber : « Zuerst wurden die künstlichen Sodaarten nur für die bunten Seifen benutzt; man gebraucht sie heute für die weissen mit demselben Erfolge, ja man gebraucht sie ausschliesslich. » (26) Was noch um diese Zeit (etwa 1860) als

(24) *Moderne Theorien der Chemie*, 2. Aufl. 1872, S. 323.
(25) Vgl. E. DARMSTÄDTER in der Lippmann-Festschrift, S. 4. (1927).
(26) In TURGAN, *Les grandes Usines de France*, Bd. 2, S. 75, 87 (1863).

LEBLANC-Soda geliefert wurde, geht aus folgender zeitgenössischer Analyse hervor :

	Natriumhydroxyd	5
	Natriumkarbonat	28
Lösliche Bestandteile	Schwefelnatrium	0.5
	Natriumchlorid	2
	Natriumsulfat	2.5
	Calciumsulfid	38
	Calciumcarbonat	15
Unlösliche Bestandteile	Schwefeleisen	2
	Silikate und Sand	5
	Kohle	2

Die viel grössere Reinheit der SOLVAY-Soda bedeutete zunächst für die Praktiker z.B. in der Glasindustrie einen Nachteil; Nebeneigenschaften, die auf dem Gehalte an Sulfat beruhten, fehlten. Aehnlich ging es mit manchen *künstlichen Teerfarbstoffen ;* HENRY E. ARMSTRONG verkündete sogar noch im Jahre 1920 die Rückkehr zum natürlichen Indigo, weil seine Begleitstoffe dem Kunstprodukte zu seinem Nachteile fehlten (27).

Dieses Beispiel kennzeichnet jene « Rückkehr zur Natur », die darin liegt, dass man besser auf die Begleitstoffe achten lernte. Die isolierten « wirksamen Prinzipien » enthalten nicht immer die g a n z e Wirksamkeit, Nebenbestandteile mit spezifischer Wirkung gingen bei der Isolierung der reinen Verbindung verloren. Besonders deutlich ist dies für die chemische Nahrungsmittelindustrie seit der Entdeckung der *Vitamine*. Das hat natürlich das Bestreben zur Folge, nun die Vitamine ihrerseits rein abzuscheiden; denn wir begnügen uns nie damit, Wirkungen festzustellen, wir sehen sie als Hinweis auf die Anwesenheit des wirkenden Stoffes an. Wir erkennen keine für sich bestehenden Einzeleigenschaften an. Immer sind sie an einen Stoff mit seiner Fülle von anderen Eigenschaften gebunden.

Man findet weitere Beispiele ungefähr in der ganzen chemischen Industrie. Nur eines noch sei herausgegriffen. BERZELIUS berichtet in seinem « Lehrbuch » (28) : Einige Chemiker wollen den

(27) *Chem. Trade Journ.* vom 17.4.1920, S. DYES, *Internationales Handbuch der Wirtschaftschemie*, Bd. 1, S. 374 f, (1921).

(28) *Lehrbuch*, Bd. 3, 2. Abteilung, S. 745, (1828). (Deutsche Ausgabe).

Pflanzenfasern « dieselbe Begierde, sich mit dem Farbstoff zu vereinigen » geben, wie sie die *Wolle* besitzt. « Zu solchem Endzweck wird zum Beispiel beim Färben von Leinen und Baumwolle mit Krapp Schafmist zugesetzt ». Der technische Erfolg blieb aus, so dass man sagen muss, « dass diese sogenannte Animalisation auf einer theoretischen Voraussetzung beruhe, die niemals gründlich untersucht worden ist und sich daher beibehalten hat ». Aber nicht der Gedanke selbst ist schlecht; man erkennt ihn in neuesten Bemühungen zur Amidierung von Baumwolle wieder (29).

Es sind verschiedene *Zerlegungsweisen* : diejenigen, die den Stoff in seine *Atome* zerteilen und diejenigen, die ihn nach seinen *Eigenschaften* zerpflücken. Das lässt sich auf den Unterschied zwischen selbständigen und unselbständigen (unabtrennbaren) Teilen zurückführen, der in der heutigen Logik wieder eine Rolle spielt. JOACHIM JUNGIUS, der Zeitgenosse BOYLE's, machte ihn an dem Beispiele des Leinengewebes klar : hypostatischer, d.h. abtrennbarer Bestandteil ist der Leinenfaden, synhypostatisch oder unabtrennbar ist die Gewebe-Eigenschaft. Zum qualitätslosen Grundbestandteil der Atome dringen wir erst, wenn wir sie in die Elektronen spalten, und dann bleibt der Atomkern mit der Anordnung der ihn umgebenden Elektronen als Eigenschaftsträger. Ein Elektron ist nicht für sich die Ursache für eine bestimmte Eigenschaft, sondern wird es erst durch seinen Verband mit den anderen Elektronen. Das abgetrennte Elektron hat andere Eigenschaften als diejenigen, die es durch diese Zusammenwirkung mit anderen erhält. So stehen wir wieder vor der Situation, die zur Abkehr von der alten Lehre der Eigenschaftselemente führte; es ist die Situation, aus der BOYLE nach der Widerlegung der aristotelischen und der peripatetischen Elemente nur den einen Ausweg sah, auf *Materie und Form* zurückzugehen.

Heidelberg. EDUARD FÄRBER.

(29) Vgl. P. KARRER u. W. WEHRLI, *Zeitschr. angew. Chemie* 39, 1509 (1926).

The early history of the first chemical reagent

The black colour produced when gallnuts are treated with salts of iron was known quite early in the history of chemistry and references to dyes and inks from misy, chalcanthum and nutgalls are frequently to be found (1). The first scientific application of this phenomenon is due to PLINY (2), who recommends the use of a papyrus treated with gallnuts for the detection of iron in verdigris. He states :—« Deprehenditur et papyro, gall prius macerato ; nigrescit enim statim aerugine inlita » (3). KOPP (4) writes enthusiastically of the reagent : « Die Galläpfel boten also das erste Reagens dar, und mittels ihrer bereitete man auch das erste Reagenspapier ». Although many ancient authors wrote about atramentum sutorium and scriptorium (5), no interest seems to have been paid to this phenomenon until ALBERTUS

(1) DIOSCURIDES, I, 146, states that galls macerated with vinegar or water turn the hair black, but he makes no reference to iron salts in this connection. The *Hortus Sanitatis* (Mainz 1485), Capt. 203, gives the following particulars :—« Item wer do wil schwartz hare machen der neme goloepfel die do dicht und swere synt und mit lochericht und syede die in oelen und syhe dan diss oele durch eyn duch und laiss ess darnach drucken werden an der sonnen und nym dan diss pulueres und syede diss mit regen wasser und wesche dyn hare do mit oder den bart er wirt schwartz. » It is noteworthy that in the chapter on galls references are made to PLINY, SERAPION and PLATEARIUS, none of whom mentions iron. The only ancient reference to the use of iron salt in this connection seems to be in the BOWER M. S., which gives a formula for a hair-dye consisting of copper sulphate and iron sulphate boiled with myrabolanes, which are rich in tannins, see RAY, *A History of Hindu Chemistry* I, p. 53, Calcutta.

(2) *Hist. Nat.*, XXXIV, 11.

(3) KOPP, *Geschichte der Chemie* II, p. 51 (1843), in transcribing wrote « illita » in place of « inlita », and this error has found its way into other writings on the history of chemistry.

(4) KOPP, *loc. cit.*

(5) See CANEPARIO, *De Atramentis*, Londini (1660), for a list of authors who dealt with the subject.

MAGNUS (6) drew attention in his botanical writings to the black colour conferred on galls by vitriol. None of the subsequent ancient writers on botany (7) refers to it, however, with the exception of CLUSIUS (8), who seems to have vaguely connected it with the fact that bread made from gallnuts produces black faeces (9). The most important event in this chapter of chemistry was the observation made by PARACELSUS (10) that gallnuts may be used as a means for the detection of iron in water. To summarise therefore, it is found that the reaction was known to PLINY, that its theoretical aspect (11) was first commented on by ALBERTUS MAGNUS and that its introduction into analytical chemistry was due to PLINY and PARACELSUS. From the time of PARACELSUS onwards galls became a reagent in water analysis, being used contemporarily with PARACELSUS by THURNEYSSER (12) and LIBAVIUS (13), both of whom acknowledge PARACELSUS.

The whole problem was attacked from quite a different point of view by TACHENIUS (14), who was mainly interested in the astringent principle present in the gall. TACHENIUS may thus

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(6) Beati ALBERTI MAGNI, Ratisbonensis episcopi, *Ordinis praedicatorum opera, quae hactenus haberi potuerunt*... Studio et labore R. A. P. T. PETRI JAMMY, Vol. IV, p. 367, Lugdini (1651).

(7) The herbals by MEGENBERG, BOCK, MATTIOLI, LONICER and the *Hortus Sanitatis* (1485) mention gallnuts but make no reference to the black colour produced by iron salts.

(8) CLUSIUS, *Rariorum aliquot stripium per Hispanias observatorum historia* I, p. 21, Antverpiae (1576).

(9) See also CASPARI BAUHINI, *ΠΙΝΑΞ Theatri Botanici*... p. 421, Basileae (1671).

(10) PARACELSUS, *Bäderbüchlein oder sechs köstliche Tractate von Wasserbädern*, Mülhausen (1562) and *De Thermis*, Colon. (1570) where several references to this reagent are given.

(11) According to MIRIAM : « The conjoint aggressive properties of iron and astringent matter manifest themselves in the black colour of their offspring. » As regards the time when MIRIAM wrote, see LIPPMANN, *Entstehung und Ausbreitung der Alchemie*... p. 43, Berlin (1919).

(12) THURNEYSSER, *Pison, von kalten, warmen, minerischen und metallischen Wassern*, Frankfurt (1572), and Strassburg (1612) pp. 17-23.

(13) LIBAVIUS, *De judicio aquarum mineralium* in I) *Commentationem metallicorum*, p. 317, Francofurti (1597); and II) *Commentariorum Alchimiae*, p. 157, Francofurti (1606).

(14) TACHENIUS, *Hippocrates Chymicus*, pp. 124-125, Brunsvigae (1666). English translation by J. W., pp. 56-61, London (1677). The original Latin is in 12°, the English translation in 4°.

be regarded as the founder of the chemistry of the tannins. The elegance with which TACHENIUS dealt with the whole question is so striking that the following of his observations and remarks, which may be described as the incunabula of the whole problem, are here reproduced :

« I have formerly told you, That neither Acid nor Salt, nor any other Sapor, doth overcome and destroy Vitriol, but Alcalyes alone, but the Juice of unripe Galls, falls in with Vitriol, and makes a coalition therewith into black, destroying and absorbing the Acidity thereof, and therefore this Juice is to be reckoned amongst Alcalyes » (15); « after this manner, Ink, and all Black Tinctures are made : yea, when the Acidity of the Vitriol is not, to suffiency, combibed by the Alcaly of the Galls; which comes to pass, when the Galls are too ripe, and are washed by the rain; then the Alcaly doth expire, as it happens to all vegetables, of which hereafter in its place; then the Vitriol erodes and eats out the Cloth, and it becomes as rotten; whence the Vulgar say, *La robba e brusada, nella tinta.* So also, Letters written with Ink, boiled with the aforesaid Galls, do wax pale, because the prevalent Acidity of the Vitriol, consumes the weak Alcaly of the Galls, being washed with the Rain, after the same manner as any Acid Spirit spread upon a writing, presently destroys the Black Colour (i.e.) the Alcaly, which another fixed Alcaly doth again recover and reduce. So if you write with water, wherein Vitriol of Iron hath been dissolved, when it is dry, no sign of any Writing

(15) It will be realised that according to TACHENIUS the astringent principle of the gallnut possesses alkaline properties, and this view was quite independently expressed by LEMERY in his *Sur la Composition des différentes espèces de Vitriols naturels, et explication Physique et Sensible de la manière dont se forment les Ancres vitrioliques*, in *Hist. de l'Acad. Roy. des Sciences*, pp. 538-549 (1707). LEMERY's alkalinity hypothesis had its followers in NEUMANN (1759), LEWIS (1765), BERGMANN (1778), GIONATTI (1779) and BERGIUS (1782), although the Appendix : *Sur le principe astringent végétal* in Vol. III, pp. 403-420 of the *Éléments de Chymie, théorique et pratique*... Dijon (1778) already ascribed acidic properties to the astringent principle of gallnuts. The acidic property of gallotannin does not however agree with the modern views on the chemistry of gallotannin as propounded by EMIL FISCHER from 1912 to 1918, with the result that most fantastic explanations have been put forward, as, for example, those by FISCHER and FREUDENBERG in the *Berichte der deutschen Chemischen Gesellschaft*, Vol. 45, p. 922 (1912). These explanations of FISCHER and FREUDENBERG for the acidic properties are as sound as the alkalinity theory of TACHENIUS and thus history repeats itself.

will appear, but if you smear over the Writing with a simple infusion of Galls, in a moment the Alcaly of the Galls makes the Writing Black, which by a powerful *Acid* or *Aqua fortis*, is presently blotted out, *viz*. The Alcalyes being consumed by the *Acid*. Again, smear over the paper with fixed Alcaly, this will again consume the *Acid*, and the *Writing* will re-appear. By what hath been said, it appears, that Vitriol doth not dye Black, unless its Acidity hath been absumed by some Alcaly or other.

« But that the Alcaly of Galls may be made more perfect, anoint them over, at least with some Fat, or with some Oil, the hidden Acidity of which enters into the Galls, being placed in a pot, in a slow Fire of Ashes, leave them there till you see the Galls become Blacker, but not so as to be reduced to Coals; then their Alcaly will be more fit for Colouring, and an Ounce of such burnt Galls, doth more than a pound of others; yea it colours of it self, because the Acidity of the Fat, by the Fire, hath acted upon its Alcaly.

« Very many Vegetables do abound with the like Volatile and Occult Alcaly, as the greater Housleek, Sage, Rinds of Pomegranates; all which do spend and absume the Acid of the Vitriol, and cause the Colcotar to be much less black...

« But you must note, that Artificial Vitriol of an Azure colour, (which is falsly called *Cyprus Vitriol*) doth not become black, with Galls, though they be burnt, but with Rinds of Pomegranates it tinges obscurely Yellow : Now it is made of the spangles or thin flakes of Copper, by Spirit of Sulphur, or of common Vitriol; both which in a cold place, are Coagulated into somewhat long-angular little stones, hardly dissolvable, and unfit for Distillation, because it wholly wants that *Cupreous Sulphur*. This, with Urine, waxeth green, and with the Alcaly of Urine, is cast into an Azure bottom, which by Fusion returns to Copper.

« So also *Verdigrease* (as proceeding from ripe Copper and Vinegar) doth not wax black with Galls, but becomes of a light red or Spadiceous colour, and, by the Reformers leave, I know, and have experimented, that nothing but Vinegar will be distilled from thence, because the remaining *Caput Mortuum*, after Distillation, by the fire of Fusion, is reduced to pure Copper, of which more hereafter.

« Whence it appears, that burnt Brass, with tosted Galls,

produceth not a black tincture, as *Alexius* of *Piedmont* (16) promises with which process the Women of this Country do wonderfully vexe themselves, that therewith they may black their hair; for as far as this composition tinges any thing, it ownes that Vertue to the tosted Galls; the burnt Brass contributing nothing thereunto ».

Independently from PARACELSUS galls were used by DUCLOS (17) for the detection of iron in water. DUCLOS was therefore generally regarded in France as having discovered this reagent. Thus FOURCROY (18), to quote from the English translation of his textbook, states : « This reagent has been known and employed with success in the analysis of mineral waters since the time that DUCLOS recommended it in 1667. » BOYLE (19), who wrote on the subject in the same year as DUCLOS, deals with it under the title « Experimental remarks upon the (usual) way of examining mineral waters by the help of galls », as follows :

« Since the change of colour, that mineral waters produce in the infusion or tincture of galls, is the most usual way, that many physicians, and the almost only, that some of them endeavour to discover or examine mineral waters by, it may be worth while, in this place, to set down some remarks, that I have made about this way of probation; and the rather, because it may, *mutatis mutandis*, be not unusefully applied to the exploring the qualities of mineral waters by colorations, though made with other materials than galls. First then it may be observed, that one need not make an infusion or tincture of galls in common water, to try if, by their means, a new colour will be produced : for I am wont to beat them to powder (20), and keep them in a glass (not

(16) TACHENIUS refers here to *Les Secrets*, Part II, fol. 47, Paris (1561), where details are given « pour faire la barbe noire, les cheveux noirs », etc.

(17) DUCLOS, *Observationes super aquis mineralibus diversarum provinciarum Galliae in Academia Scientorium Regia in annis* 1670 *et* 1671 *factae, et Dissertatio super principiis mixtorum materiallum habita* 1677. Lugd. Batv. (1685). FOURCROY's statement that DUCLOS had recommended the reagent in 1667 apparently refers to an earlier paper by DUCLOS which I have not seen.

(18) FOURCROY, *Elements of Natural History and of Chemistry*, Vol. III, p. 478, London (1787).

(19) BOYLE, *Short Memoirs for the Natural Experimental History of Mineral Waters*, London (1685). Collected Works, Vol. IV, pp. 231-250, London (1744).

(20) After BOYLE the use of powdered galls became quite general, although it is noteworthy that as late as 1808 PFAFF, *Journ. Chem. und Phys.* v, 331, regarded it as a novelty.

too big) exactly stopped, by which means I have them always in readiness to mingle with the mineral water, and alter the colour of it, if galls be able to do it, almost in a trice; whereas, to draw the tincture of galls with simple water, often takes up several hours and the tinging parts are much weakened by being diluted by the menstruum. If you would have a tincture, the powder of galls, tied up close in a rag, and with it hung in the liquor, makes the infusion less muddy. If you be in haste, and have none of the powder in hand, you may scrape as much of a gall-apple, as you need, into the mineral water.

« 2. I have observed those parts of the infusion of galls, (especially if made by heat) that produce the new colour with ferruginous waters, to be more apt to fly away than one would think, the infusion becoming often unfit to alter the colour of martial waters, whilst yet itself appears sufficiently high coloured. Upon which account I chuse to make a tincture of galls not long before I mind to use it; and if I employ dry galls, to take powder, that is not stale.

« 3. It is no safe way, and may be very erroneous, that is usually taken in mixing galls, or their infusion with the water to be explored, so carelessly, as is wont to be done. For those, that are curious to make good ink, will easily believe, that much of the deepness of the colour depends upon the proportion of galls to the other ingredient; and accordingly, that by putting a much greater, or a much lesser, quantity of galls into such a quantity of the mineral water, the resulting colour may be more or less intense. To obviate which inconvenience, I take this course, when the occasion deserves it; I make my infusion of galls with a certain weight of the powder in a determinate weight of water. As for instance, I put about five gr. of powdered galls, to steep for so many hours in an ounce of water : but if I make use of the dry powder, then I am wont to put three or four grains into an ounce of the liquor to be examined; which is a way far more certain, than the common, wherein the ingredients are estimated but by guess. I have mentioned various proportions of powdered galls to the same quantity of liquor, because I have observed, that there is really a great inequality among the mineral waters, in which it may be put; and I have found by trial, that in an ounce of the *German* Spa, a single grain of powder would immediately produce a sufficiently deep purple colour.

« It is an inconvenience, that not only galls, but the other drugs hereafter to be mentioned, impart a high tincture of their own to the common water they are infused in; and therefore it were to be wished, and is fit to be endeavoured, that we had some drug, that, without imparting a colour to the common water it impregnates, would afford an infusion fit to strike a blackish or a purple colour with martial waters.

« Though it be useful, yet it is not necessary, to employ galls to produce a colour in the mineral water proposed; for besides that it is known, that usually, (though not always, as I have tried) the same thing may be done, but somewhat more faintly, with oaken leaves, we may successfully enough substitute, for the same purpose, some other astringent vegetables, as dried red-rose leaves, the peel, and (as we have tried) the juice of pomegranates; and (what I find to be a notable stiptick) the blossoms of the same plant, (which are vulgarly called in the shops *ballaustium* :) to which may be added myrobolans, logwood, and some others, that need not now be mentioned, whose strong infusions have yielded me a tincture very dark and blackish with some martial liquors ».

BOYLE's observations were soon afterwards extensively made use of by LEIGH (21), VICARIUS (22) and NEUMANN (23). The latter used BOYLE's experience in a manner which is most attractively told of by THOMSON (24) :

« Some merchants in Holland, England, Hamburg, and Dantzic, were in possession of what they considered an infallible test to distinguish French brandy from every other kind of spirit. It was a dusky yellowish liquid. When one or two drops of it were let fall into a glass of French brandy, a beautiful blue colour appeared at the bottom of the glass, and when the brandy

(21) LEIGH, *Tentamen de aquis mineralibus*, London (1694).

(22) VICARIUS, *Hydrophylacium novum, seu discorsus de aquis*... Ulmae Svenorium (1699). VICARIUS assigns (p. 70) the discovery of the reagent to PARACELSUS and THURNEYSSER and makes no reference to BOYLE. Similarly LENTILIUS, in his *Miscellanea curiosa*... Observ. 201 (1686) mentions PARACELSUS only as having discovered this test.

(23) NEUMANN, *De experimento probandi spiritum vini Gallici, perquam usitato, sed revera falso et folici*, No. 391, in the *Trans. Royal Society of London*, pp. 398-408 (1724-1725).

(24) THOMSON, *History of Chemistry*, Vol. I, p. 264, London (1830).

is stirred, the whole liquid becomes azure. But if the spirit tried be malt spirit, no such colour appears in the glass. NEUMANN ascertained that the test liquid was merely a solution of sulphate of iron in water, and that the blue colour was the consequence of the brandy having been kept in oak casks, and thus having dissolved a portion of tannin. Every spirit will exhibit the same colour, if it has been kept in oak casks ».

In addition to the workers mentioned galls were used for the detection of iron in water also by HIERNE (25), HOFFMANN (26) BRANDT (27), TEICHMEYER (28), WALLERIUS (29), BERGMANN (30), STRUVE (31), BRUGNATELLI (32) and many others (33).

In conclusion it is perhaps interesting to state that ELLER (34) used gallnuts for the detection of iron in the calx of human blood, thereby proving for the first time the presence of iron in blood.

The University, M. NIERENSTEIN.
Bristol, England.

(25) HIERNE, *Brevis manuductio ad fontes, et aquas minerales*.. .Holmiae (1707).

(26) HOFFMANN, *Acta Laboratorii chymici Aldorfini* Norimb. (1719).

(27) BRANDT in *Acta Litt. et Scien. Sulliae*, Vol. III, pp. 39-43 (1723). See also CRELL's *Neues chemisches Archiv* 1784 pp. 274-279. BRANDT expresses the hope that a test for arsenic will be found which will compare with the reliability of the gallnut test for iron.

(28) TEICHMEYER, *Institutiones chymiae*... Ienae (1729).

(29) WALLERIUS, *Hydrologie, nebst Anleitung zur Anstellung von Wasserproben*... Berlin (1751).

(30) BERGMANN, *De analysi aquarum*, Upsala, (1778).

(31) STRUVE, *Von den Reagentien und ihrem Gebrauche bey der Zerlegung der Mineralwasser*, Jena (1786).

(32) BRUGNATELLI, *Elementi di Chimica*... Pavia (1795). CRELL, who reviewed the book in the *Chemische Annalen* for 1796, Part I, p. 661, points out that BRUGNATELLI does not mention that BERGMANN had discovered the test !

(33) GMELIN, Geschichte der Chemie, Vol. I, pp. 740-790, Göttingen (1798), gives a large number of publications which deal with water analysis. Of these I have had the opportunity of examining some 30 odd, in all of which I have found that iron is detected with powdered gallnuts as recommended by BOYLE.

(34) ELLER, in *Hist. de l'Acad. Royale de Berlin*, 1751, p. 11.

Notes and Correspondence

I. Miscellanea.

Prospectus for a corpus of medieval scientific literature in Latin. — In my « Prospectus for a corpus of medieval scientific literature in Latin, » (*Isis*, 14, 1930, 368-384) at page 381, item 164 is misprinted, « Basel. Ioh. Commentary on the Almagest, » instead of RASEL. IOH. etc. However, this is just as well because the author of this commentary on the *Almagest* is not JOHN RASEL, but GEORGE OF TREBIZOND or Trapezuntos, the Greek scholar who came to Italy and translated for pope NICHOLAS V. I followed the catalogue description of the work as it exists in MS Laud. Latin 111, 15th century, fols. 153, codex chartaceus, quondam D. GRIMANO cardinalis S. Marci ; olim 651, « *In C. Ptolemaei Magnae Constructionis sive Almagesti libros tredecim commentarii per Joh. Rasel cum introductione.* » And indeed in the manuscript itself there is no mention of GEORGE OF TREBIZOND, while the work closes,« ... *res ipsa me coget si eo praesente non potero absente saltem quam plurimis doctis hominibus convocatis meum adversus ignorantes honorem defendere. Laus Deo. Ioh. Rasel* » (or, Raesel).

A further examination of the treatise itself, however, of the dedicatory letter to JACOBUS ANTONIUS MARCELLUS and a letter at its close to NICHOLAS V, and comparison of these with what TIRABOSCHI, *Storia della letteratura italiana*, edition of 1824, VI, 1 (or, VII), 531-532, says of the Ambrosian codex of GEORGE OF TREBIZOND's work, makes it clear that the commentary in the Laud manuscript is that of GEORGE OF TREBIZOND. He boasted of finishing it in nine months and the translation of the text of the *Almagest* in nine months and twenty-two days. But his work was severely criticized by JAMES OF CREMONA, to whom NICHOLAS V had submitted it, and this appears to have cost the author the papal favor and employment which he had enjoyed. Since the criticisms made by JAMES seem to have been well taken, this commentary by GEORGE OF TREBIZOND should be omitted from the *Prospectus*.

LYNN THORNDIKE.

Prix Binoux 1930. — L'Académie des Sciences a décerné cette année deux prix Binoux pour l'histoire et la philosophie des sciences. L'un d'eux a été attribué à M. PIERRE BRUNET, professeur au lycée de Joigny, France pour les ouvrages suivants : Les physiciens hollandais et la méthode expérimentale en France au XVIII[e] siècle (*Isis*, 10, 173-74), et : Maupertuis, étude biographique, l'œuvre (*Isis*, 15, 177-79). L'autre a été donné à M. NIELS NIELSEN, professeur à l'Université de Copenhague, pour son œuvre : Géomètres français sous la Révolution, VIII+250 p., LEVIN et MUNKSGAARD, Copenhague, 1929. (*Isis*, 16, 504.)

L. G.

II. Personalia.

Josef Drecker. — Heute nachmittag 3,15 Uhr entschlief sanft nach kurzem Leiden unser lieber Bruder, Schwager und Onkel Prof. Dr. JOSEF DRECKER im 79. Lebensjahr, versehen mit den hl. Sterbesakramenten der röm.-kath. Kirche. Im Namen der trauernden Hinterbliebenen :

Familie URBAN DRECKER

Dorsten i. Westf., den 24. März 1931.
Markt, 9.

Die Beerdigung findet am Samstag, dem 28. März 1931 nachmittags 2 Uhr, in Aachen von der Leichenhalle des kath. Friedhofes, Vaelserstr. aus statt.

Noufumi Okamoto. — Mr. NOUFUMI OKAMOTO died on February 17, 1931, at the advanced age of 84. He was a very devoted student of old Japanese mathematics, but was averse to publishing the results of his investigations. At the time of his death he was preparing a catalogue of the rich collection of old Japanese mathematical books owned by the Imperial Academy of Tôkyô.

YOSHIO MIKAMI *(Tôkyô)*.

Reviews

Adrien Favre. — *Les Origines du Système Métrique* 242 pages. Paris, Les Presses Universitaires de France, 1931.

Ce nouveau livre sur le système métrique est l'ouvrage de M. Adrien Favre, Professeur au Lycée de Toulouse. Il est divisé en quatre parties : *Un problème administratif*, *Un problème scientifique*, *La première solution*, et *La solution définitive*,et traite clairement le sujet du point de vue français. Les détails donnés témoignent d'une connaissance des sources qui ne vient qu'après beaucoup de recherches. Il y a aussi une bonne bibliographie.

Le plan choisi par l'auteur n'est pas le plus simple. La distinction entre le problème scientifique et le problème administratif est arbitraire et entraîne beaucoup de répétitions quand les sujets sont traités chronologiquement comme ils le sont ici.

Une omission assez grave, c'est qu'il n'est pas fait mention de l'œuvre remarquable de Simon Stévin, qui en 1585 a publié en français aussi bien qu'en flamand la *Disme* ou la *Thiende* où il enseignait déjà l'usage du système décimal et son application au système de poids et mesures. M. Favre se borne à nous dire : « C'est le développement d'une idée, née probablement en Angleterre, reprise en France par Mouton, Picard, par le Hollandais Huygens — puis au Danemark par Roemer — etc. » (p. 217).

Il serait peut être injuste de reprocher à l'auteur que son étude des efforts américains est incomplète. Il en indique les sources assez vaguement comme suit : « Pour les détails concernant les Etats-Unis, voir les *Œuvres* de Jefferson *(Writings of-)*, en particulier (t. VII, p. 473) son rapport présenté le 13 juillet 1790, et l'étude de Louis-A. Fischer, *History of the Standard of Weights and Measures of the United States*, publiée par le *Bureau of Standards* de Washington. » (p. 109, n. 1.) C'est dommage qu'il n'ait pas donné la date des œuvres de Jefferson afin de faciliter la vérification. La brochure de M. Fischer contient un bon résumé des principaux faits, mais il n'a pas remonté aux sources pour raconter les origines.

La citation de Talleyrand en tête du livre donne l'impression qu'il

appelait le système métrique « ... une entreprise dont le résultat doit appartenir un jour au monde entier. » TALLEYRAND dans sa « proposition » a parlé ainsi de la réforme des poids et mesures avant que la notion de la division décimale ne fut introduite et avant qu'il n'y ait eu un vrai *système métrique*. La note 2 au bas de la page 110 cite une lettre de JEFFERSON à William *Scott*, le 26 juillet 1790. Je crois que cette lettre était adressée à WILLIAM SHORT, mais ne connaissant pas l'édition que M. FAVRE a employée, il ne m'est pas possible de vérifier cette assertion.

Ces petits détails inexacts tendent à nous faire perdre confiance dans le livre de M. FAVRE, mais nous aurions mauvaise grâce à ne pas lui reconnaître des qualités qui compensent ces défauts.

C. D. HELLMAN.

Sir Thomas L. Heath. — *A manual of Greek mathematics.* XVI+552 p., 2 fig., Oxford, Clarendon Press, 1931 (Oxford University Press, 114 Fifth Avenue, New York, $ 5).

Thanks to the efforts of a galaxy of great historians such as CANTOR, HEIBERG, ZEUTHEN, TANNERY, LORIA, and HEATH, the history of Greek mathematics is already very well known or, to put it more correctly, the available materials have been worked out almost as fully as one might expect, and their analytical study has been completed by various syntheses correcting one another in such manner that we have (or believe we have) a fair understanding of the subject. The probability of discovering new Greek texts is not very great, though optimists may derive much encouragement from the fact that ARCHIMEDES' Method was brought to light hardly more than a quarter of a century ago (HEIBERG, Constantinople, 1906). There are two fields however out of which one may reasonably expect to dig out many more treasures. The first is that of Egyptian and Mesopotamian archaeology, and my metaphor is particularly apt with reference to it, for the documents have actually to be dug out of the ground. The second is that of Arabic mathematics, for the investigation of Arabic MSS. is still so incomplete that it would not be very surprising if some lost Greek treatises were eventually found in the form of Arabic versions or commentaries.

The present volume is essentially a condensation of Sir THOMAS' *History of Greek mathematics* (2 vols., Oxford, 1921) which was fully reviewed in this journal (*Isis*, 4, 532-35). The plan of both works is essentially the same, though the aim is somewhat different. Says the author (p. V) :

« In the *History* I had in mind the requirements, on the one hand, of the classical scholar who might look for light on the interpretation of passages of mathematical content in Greek authors which came his way, and, on the other hand, of the

expert mathematician who might wish so far to assimilate the whole argument of a particular treatise, say of ARCHIMEDES, as to be able, on occasion arising, to apply the same method to a different problem. But besides the categories of classical scholars and professional mathematicians there is the general reader who has not lost interest in the studies of his youth. »

Moreover Sir THOMAS has taken full advantage of the investigations made during the last ten years. These investigations refer mainly, if not exclusively, to the two fields above mentioned. With regard to the first field, he has been able to use the new editions of the Papyrus Rhind by T. ERIC PEET (1923 ; *Isis*, 6, 553-57)and by ARNOLD B. CHACE (1927-29 ; *Isis*, 14, 251-55), but not V. V. STRUVE's edition of the Moscow Papyrus (1930 ; *Isis*, 16, 148-55) ; also the decipherment and discussion of Babylonian tablets by E. F. WEIDNER, A. UNGNAD, H. ZIMMERN, C. FRANK, V. V. STRUVE, and chiefly by O. NEUGEBAUER ; there is also (on p. 530) a brief and second hand reference to the newer studies of Babylonian astronomy. (See for example, PAUL SCHNABEL, 1926 ; *Isis*, 10, 107). With regard to the second field, he has integrated the results published by the late CARL SCHOY in *Isis* under the title *Gaeco-arabische Studien* (vol. 8, 21-40, 1926), notably ARCHIMEDES' method of constructing a regular heptagon, as preserved in Arabic translation by THÂBIT IBN QURRA.

All of which is all right as far as it goes, but is not yet entirely satisfactory. I believe that in any history of Greek mathematics, irrespective of size, one or two introductory chapters should be devoted to Egyptian and Mesopotamian mathematics. This is indispensable to show the development of Greek mathematics in its right perspective. The first chapters of HEATH's *History* and of his *Manual* ought to be completely recast, and the materials contained in the appendices should become integral parts of these chapters together with much additional information. After all appendices are only makeshifts. Let us hope that the success of these volumes will be sufficient to warrant the making of these radical alterations in subsequent editions.

The practical value of the *Manual* is much enhanced by good English and Greek indices.

GEORGE SARTON.

Cassius Jackson Keyser. — *Humanism and Science.* VII+243 p., New York, Columbia University Press, 1931. ($ 3.00).

George Sarton. — *The History of Science and the New Humanism.* 178 p., New York, HENRY HOLT and Co., 1931. ($ 2.00).

Humanism is a word of many implications. It is used as a shibboleth

by those who decry the encroachments of science upon arts and letters and it is employed by those who hold that the modern revelation of nature is an impressive agent for the spiritual amelioration of society. Clearly there is a need for definition of terms and a frank discussion of their implications. The two books under review provide this important need.

The book of Professor KEYSER is devoted to a definition of terms in so far as this is possible and an appraisement of humanism in consequence of this definition ; that of Doctor SARTON to the thesis that science, particularly as it is revealed in historical development, is a thoroughly humanistic discipline, which, far from being antagonistic to the cultural and spiritual progress of mankind, is perhaps its chief stimulus.

Professor KEYSER adopts as his definition of Humanism the words of WALTER LIPPMANN : « Humanism signifies the intention of men to concern themselves with the discovery of a good life on this planet by the use of human faculties. »

In the development of this idea the author assumes that « (the limitless ameliorability of human well-being) is at once the origin and the support of the great dream of modern Humanism—fairest of all dreams—the dream of world unity, world solidarity and world coöperation in the achieving of a good life for all mankind. The aim... is to assess, in a large way and in general terms, the worth of Science regarded as an agency for advancing, continually and endlessly, approximate realization of that dream. »

The book of Professor KEYSER is divided into five parts. In the first of these he attacks with vigor several kinds of pseudo-humanism, particularly that type of humanism in which science is excluded by those who fail to see its capacity for inspiration. The second part is devoted to definitions of humanism ; the third to a comparison between mathematics and science ; the fourth and fifth to an exploration and appraisement of the humanistic values in mathematics and science.

In order to understand his position we must first understand his view with regard to the nature of both science and mathematics. His first contention is that neither is a part of the other, but that they are complementary disciplines.

« Science, » says Professor KEYSER, « is the enterprise having for its aim to establish Categorical propositions ; in other words, it is the enterprise having for its aim to answer questions relating to the Actual world... Mathematics is the enterprise having for its aim to establish Hypothetical propositions ; in other words, it is the enterprise having for its aim to answer questions relating to the world of the Possible ».

All types of knowledge are embraced by these two classes according

to Professor KEYSER, and he illustrates this important point by showing the categorical and hypothetical elements in such non-scientific subjects as law and ethics. Law (judicial behaviour) according to the author may be regarded as a functional depending upon several variables which may be designated in the familiar notation of mathematics as L (a, b, c,...), where a, b, c, etc. are variables which might represent (a) modes and forms of business, (b) manners, customs, mores, (c) religious opinion and feeling, etc. The actual form of the functional L is not known *a priori*, but must be determined by observation exactly in the manner that the functions of physics are determined by experiment. This determination is the business of science ; the hypothetical propositions which may then be deduced from it are obtained by means of mathematics in its broadest sense.

If one admits the premises of Professor KEYSER and accepts his definitions, then one is forced to admit an unusual measure of universality to mathematics and science. It is quite possible that the unfortunate hostility between the literary humanists and the scientific humanists may be due to a misunderstanding of this point, or perhaps rather to an unwillingness of the former to admit its validity.

Very similar in its general point of view is the volume of Doctor SARTON, which, one should note, is published as the 1930 contribution to the Colver lectures of Brown University. It is divided into three essays : The History of Science and the History of Civilization ; East and West ; The History of Science and the New Humanism. In general the thesis of this book is that humanism is very old and that the new world must not be too arrogant in its possession of riches, not the west too unmindful of the great inheritance which it has received from the east.

The reviewer feels that there will be a cry of protest from the literary humanists when the full force of Doctor SARTON's claims for science and the history of science is generally realized. The author asks in the beginning of his book : « Which is the purpose of mankind ? Is such a question too ambitious ? Is it at all possible to answer it ? » and he reaches the conclusion that one can answer in the affirmative. « To put it briefly we might say that, as far as we can discern, the main purpose of man is to create such intangible values as beauty, justice, truth. » The author then inquires what assets of civilization show accumulative progress and he asserts : that « the scientific activity is the only one which is obviously and undoubtedly cumulative and progressive ». He compares the history of art and the history of science and concludes that these two great human activities possess essentially different aspects. « When one reads the history of science one has the exhilarating feeling of climbing a big mountain. The history of art gives one an altogether

different impression. It is not at all like the ascension of a mountain, always upward whichever the direction of one's path ; it is rather like a leisurely journey across a hilly country... An erratic succession of climaxes and anticlimaxes the amplitude of which cannot be predicted. This history makes one think of a rhythmic motion, or rather of many rhythms capriciously interwoven. For example, our artistic sensibility passes periodically from romanticism to classicism, or else from naturalism to idealism. »

One should note in this connection the genuine contrast between the optimism of Doctor SARTON and the pessimism shown by EMERSON in what is perhaps the most melancholy dictum on record : « Society never advances. It recedes as fast on one side as it gains on the other. Its progress is only apparent, like the workers of a treadmill. It undergoes continual change ; it is barbarous, it is civilized, it is Christianized, it is rich, it is scientific ; but this change is not amelioration. For everything that is given, something is taken. » *(Essay on Self-reliance)*.

In support of his claims for science as the great cumulative power behind the progress of man Doctor SARTON traces in brief outline the history of the shifting centers of culture, first in Egypt and Mesopotamia, then in Greece, then among the Arabs, then among the western Christians, and he notes that each of these developments leaves science higher and mankind richer in knowledge, beauty and power over nature.

The author in his third essay points out the inevitable conclusion which must follow from his premises. If we wish to know the meaning of the word humanism we must include in our study a course in the history of science. No longer must we be content to learn about the pageantry of kings, the succession of wars, and the terms of treaties. « Much of the historical writing of the past, and not a little of the present, strikes me as being a sort of gossip, a sort of exalted gossip if you please... The early historian mistook kings for leaders and stewards for creators, he was more concerned with war than with peace and with disease than with health. His accounts were anecdotic and pathological ».

To the scientist at least the author makes a convincing case by citing the two most important events of the year 1686, the publication of NEWTON's *Principia* and the constitution of the League of Augsburg. The latter, essentially unimportant to us today, was the sensation of that time, but the former, little known in its day, has become « the foundation stone of modern thought ».

One leaves two books of the kind under review with a new appreciation of the dignity and beauty of science. He is frequently reminded in both of them of the inspired words of POINCARÉ : « The scientist does not study nature because it is useful ; he studies it because he

delights in it, and he delights in it because it is beautiful. If nature were not beautiful, it would not be worth knowing, and if nature were not worth knowing, life would not be worth living. Of course I do not here speak of that beauty which strikes the senses, the beauty of qualities and of appearances ; not that I undervalue such beauty, far from it, but it has nothing to do with science ; I mean that profounder beauty which comes from the harmonious order of the parts and which a pure intelligence can grasp. This it is which gives body, a structure so to speak, to the irridescent appearances which flatter our senses, and without this support the beauty of these fugitive dreams would be only imperfect, because it would be vague and always fleeting. ».

H. T. DAVIS.

Tobias Dantzig. — *Number, the language of science.* A critical survey written for the cultured non-mathematician. VIII+260 p. New York, MACMILLAN, 1930. ($ 3.50.)

This book was written « to restore the cultural content (of mathematics) and present the evolution of number as the profoundly human story which it is ». We may say at once that the author has accomplished his main purpose, and we are very thankful to him for it. He explains it so clearly (on p. 180) that it is worthwhile to reproduce his own words ; I do this with the more pleasure because I have often expressed similar ideas though never so well.

« The systematic exposition of a textbook in mathematics is based on logical continuity and not on historical sequence; but the standard high-school course, or even the college course, in mathematics fails to mention this fact, and therefore leaves the student under the impression that the historical evolution of number proceeded in the order in which the chapters of the textbook were written. This impression is largely responsible for the widespread opinion that mathematics has no human element. For here, it seems, is a structure that was erected without a scaffold : it simply rose in its frozen majesty, layer by layer ! Its architecture is faultless because it is founded on pure reason, and its walls are impregnable because they were reared without blunder, error or even hesitancy, for here human intuition had no part ! In short the structure of mathematics appears to the layman as erected not by the erring mind of man but by the infallible spirit of God. The history of mathematics reveals the fallacy of such a notion. It shows that the progress of mathematics has been most erratic, and that intuition has played a predominant rôle in it. Distant outposts were acquired before the intermediate territory had been explored, often even before the explorers were aware that there was an intermediate territory. It was the function of intuition to create new forms; it was the acknowledged right of logic to accept or reject these forms, *in whose birth it had no part*. But the decisions of the judge were slow in coming, and in the meantime the children had to live, so while waiting for logic to sanctify their existence, they throve and multiplied. »

The whole account is very well written with a view to keeping the reader's attention on the alert, the unavoidable austerity of the subject being judiciously relieved with well chosen anecdotes (e.g., the amusing story of English tally-sticks as told by DICKENS). The titles are tantalizing, though mathematicians will easily guess what they stand for : 1. Fingerprints ; 2. The empty column ; 3. Number lore ; 4. The last number ; 5. Symbols ; 6. The unutterable ; 7. The flowing world ; 8. The act of becoming ; 9. Filling the gaps ; 10. The domain of number ; 11. The anatomy of the infinite ; 12. The two realities. (By the way, the two realities, are the commonplace reality and the transreality of modern science in the Einsteinian and Eddingtonian sense).

It is unnecessary to summarize the book—it explains the main steps in the gradual elaboration and enrichment of the number concept,—but we shall try to give the reader an idea of it by means of a few samples. The chapter on symbols begins auspiciously with a very striking saying ascribed to the great electrician HEINRICH HERTZ (p. 76) :

« One cannot escape the feeling that these mathematical formulae have an independent existence and an intelligence of their own, that they are wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them. »

The author illustrates by means of simple examples the true implications of symbolism. « *The symbol is not a mere formality* ; it is the very essence of algebra ». (p. 80). « What distinguishes modern arithmetic from that of the pre-Vieta period is the changed attitude towards the ' impossible ' » ... « Today we know that possibility and impossibility have each only a relative meaning ; that neither is an intrinsic property of the operation but merely *a restriction which human tradition has imposed on the field of the operand*. Remove the barrier, extend the field, and the impossible becomes possible. » (p. 89).

He interestingly contrasts the rigor of the infinitesimal analysis of the Greeks (the Eudoxian method of exhaustion) with the rough-and-ready methods of the seventeenth century mathematicians. « They dealt with infinitesimals as fixed or variable according to the exigencies of the argument ; they manipulated infinite sequences without much rhyme or reason ; they juggled with limits ; they treated divergent series as if these obeyed all rules of convergence. They defined their terms vaguely and used their methods loosely, and the logic of their arguments was made to fit the dictates of their intuition. In short, they broke all the laws of rigor and of mathematical decorum. » When one bears in mind that these mathematicians were actually brought up on Greek geometry, how was their laxity conceivable ? I had often asked myself that question, and was pleased to find here at least a partial explanation.

I believe that DANTZIG has put his finger on the main cause of that evil. The enormous discontinuity between the method of exhaustion which the seventeenth century mathematicians knew and the methods which they practised was due to the development of scholasticism, which had introduced a perverse rigor of its own and had thus adulterated the stream of ancient thought. « To minds whose logic was fed on such speculations as Sacrament and Atonement, Trinity and Trans-substantiation, the validity of infinite processes was a small matter indeed ». (p. 130). Or to reverse the argument, Bishop BERKELEY exclaimed in his « Analyst, or, a discourse addressed to an infidel mathematician » (p. 12, London, 1734) (1) : « He who can digest a second or third Fluxion, a second or third Difference need not, methinks, be squeamish about any Point in Divinity ». Some theologians of our own day exploit the paradoxes and difficulties of modern physics in the same vein to achieve similar ends.

From the fifth century B. C. until almost our own time the bottom of the whole trouble was that the mathematician could make no progress whatever without the concept of infinity ; he could not completely grasp but only get some conflicting and confusing glimpses of that unmanageable concept and yet he could not throw it into the outer darkness without condemning himself to impotence. As late as 1826, ABEL wrote to his old teacher HOLMBOE (2) :

« Les séries divergentes sont en général quelque chose de bien fatal, et c'est une honte qu'on ose y fonder aucune démonstration. On peut démontrer tout ce qu'on veut en les employant, et ce sont elles qui ont fait tant de malheurs et qui ont enfanté tant de paradoxes. Peut-on imaginer rien de plus horrible que de débiter $0 = 1 - 2^n + 3^n - 4^n +$ etc., n étant un nombre entier positif ? Enfin mes yeux se sont dessillés d'une manière frappante, car à l'exception des cas les plus simples, par exemple les séries géométriques, il ne se trouve dans les mathématiques presque aucune série infinie dont la somme soit déterminée d'une manière rigoureuse, c'est-à-dire que la partie la plus essentielle des mathématiques est sans fondement. Pour la plus grande partie les résultats sont justes, il est vrai, mais c'est là une chose bien étrange. Je m'occupe à en chercher la raison, problème très intéressant. »

The following quotations will illustrate the author's wisdom and style.

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(1) Apropos of which see FLORIAN CAJORI's excellent book, *A history of the conceptions of limits and fluxions in Great Britain from* NEWTON *to* WOODHOUSE (Chicago, 1919; *Isis*, 5, 156-57).

(2) The quotation given by DANTZIG (p. 161) is incorrectly dated, 1828 instead of 1826, and incorrectly translated. I prefer to give the text contained in the *Œuvres complètes* de N. H. ABEL, nouvelle édition publiée aux frais de l'état norvégien par MM. L. SYLOW et S. LIE (vol. 2, p. 256, Christiania, 1881).

After having applied the DEDEKIND « Schnitt » to time, and shown that « now » is a « cut » which separates all the past from all the future, he adds (p. 176) :

« Furthermore, paradoxical though this may seem, *the present is truly irrational* in the DEDEKIND sense of the word. For while it acts as the partition it is neither a part of the past nor a part of the future. Indeed, in an arithmetic based on pure time, if such an arithmetic were possible at all, it is the irrational which would be taken as a matter of course, while all the painstaking efforts of our logic would be directed toward establishing the existence of rational numbers. »

Finally apropos of relativity (p. 232).

« A principle of relativity is just a code of limitations : it defines the boundaries wherein a discipline shall move and frankly admits that there is no way of ascertaining whether a certain body of facts is the manifestation of the *observata*, or the hallucination of the *observer*. A principle of relativity is an act of resignation, and a philosophical principle of relativity would consist in the frank admission of the insolubility of the old dilemma : has the universe an existence *per se* or does it exist only in the mind of man ? To the man of science, the acceptance of the one hypothesis or the other is not at all a question of ' to be or not to be '; for from the standpoint of logic either hypothesis is tenable, and from the standpoint of experience neither is demonstrable. So the choice will forever remain a matter of expediency and convenience. The man of science will act *as if* this world were an absolute whole controlled by laws independent of his own thoughts or acts; but whenever he discovers a law of striking simplicity or one of sweeping universality or one which points to a perfect harmony in the cosmos, he will be wise to wonder what rôle his mind has played in the discovery, and whether the beautiful image he sees in the pool of eternity reveals the nature of this eternity, or is but a reflection of his own mind. »

It is a thousand pities that a book which was so well thought out and so well written should be disfigured by little vulgarities for the sake of « snappiness », by barbaric spelling of Arabic names, and by many historical inaccuracies. The author does not quote any sources, and that is all right considering his purpose, but errors are inexcusable. For example he writes (on p. 21) : « The oldest records indicating the systematic use of written numerals are those of the ancient Sumerians, Egyptians and Chinese. They are all traced back to about the same epoch, around 3500 B. C. » We know nothing of Chinese mathematics of that time. On p. 116, the discovery of the method of exhaustion is ascribed to ARCHIMEDES. It was discovered by EUDOXUS, and admirably used by EUCLID. On p. 193, we are told that « The great APOLLONIUS wrote a treatise on conics of which only fragments have come down to us. » (!!) To be sure such mistakes do not affect the author's argument, but their accumulation is irritating, and the more so because it would have cost so little effort to remove them.

The claim that the book is written for the « cultured non-mathematician » is, as such claims generally are, somewhat misleading. The extracts quoted by me are perhaps sufficient proof. Will the non-mathematician appreciate them, or would he appreciate them in their context? Will he understand such remarks as these : « The singularities of a curve apprise us more about its true nature than its shape does » (p. 180). « The famous identity discovered by DE MOIVRE showed the rôle the complex number plays in trigonometry while EULER amplified DE MOIVRE's formula by bringing in the transcendental *e* » (p. 185). It is true the author might try to disculpate himself by observing that for his book as well as for every other each reader will get out of it just as much as he deserves and nothing more, yet I am afraid that few « cultured non-mathematicians » will read and enjoy the whole of it.

The book contains a fine gallery of portraits : LEIBNIZ, FERMAT, POINCARÉ, EULER, ABEL, NEWTON, GEORG CANTOR, DESCARTES, GAUSS, GALILEO, KRONECKER. I have mentioned them in their order of appearance which is justified by the context. The choice is good, but I missed one portrait, — RICHARD DEDEKIND's.

GEORGE SARTON.

John Edgar Teeple (1874-1931). — *Maya astronomy* (Contributions to American Archaeology, no. 2). Reprint from Publication no. 403 of Carnegie Institution of Washington, p. 29-115, 19 fig., quarto, 1930.

A critical study of this memoir would require considerably more time and energy than is available to me at present or will be available to me in the near future, and yet I am anxious that the readers of *Isis* should know of it as soon as possible. The best will perhaps be to let the author speak for himself.

Here are his main conclusions as expressed on p. 110-111.

« For 522 years, from 8.16.0-0-0 to 10.2.10-0-0, they recorded the age of the moon at a great many intervening dates. These records show both contemporaneous agreements between cities and chronological agreements between early and late dates, so that for any newly discovered dated monument from Mexico, Honduras, British Honduras or Guatemala, we are prepared to predict the moon age recorded, with an error of not over a couple of days. The only real exception is the city of Quirigua for the last twenty years of its existence. Apparently all Maya peoples who erected monuments used an identical calendar, and it suffered no interruption during these 522 years, except possibly temporary ones due to human frailty, such as we make when we can't remember whether today is Friday the 30th or Saturday the 31st. For about 110 years of the whole period we can also predict the moon numbering in the lunar year. From 9.12.15-0-0 to 9.16.5-0-0, 70 years, it was uniform everywhere; then Copan adopted a moon eclipse system which we can closely predict, and Quirigua reverted to a previous

Palenque system beginning the lunar year one moon later than the other cities, which we can also predict.

We know their idea regarding average length of a moon for purposes of computation at three different periods. First, at Palenque when the Initial Series were written probably not long before 9.13.0-0-0, it was 29.53086 days, the formula being 81 moons = 2392 days. Second, at Copan at least after 9.13.0-0-0, and probably at all cities using the uniform system of moon numbering it was 29.53020 days, from the formula 149 moons = 4400 days. Finally, later at the time of the *Dresden Codex* the Palenque formula had been resumed.

We know something of their ideas regarding the length of the tropical year. At Copan after 9.14.0-0-0 it was 365.2420 days computed by the formulae 19 years = 235 moons and 149 moons = 4400 days. This is just a little shorter than our present-day computation, and still shorter than the Gregorian year of 365.2425 days. At Palenque, in the later years before 9.13.0-0-0, it was about 365.2430 days, probably from the formula 144 years = 146 tuns or 144 vague years, plus 35 days. This is a little longer than the Gregorian year, and seems to be about identical with the one used at Copan about the same time. The earliest determinations at Palenque seem to show a year a little shorter than the Gregorian. One gathers the impression at Palenque that they were weighing the comparative merits of a number of successive formulae, among them being one that nearly corresponds to our Julian year. Isolated examples from Yaxchilan, Piedras Negras, El Cayo, etc., indicate that all the cities probably knew a tropical year of the approximate accuracy of our Gregorian year. Finally, for a period of about 25 years after 9.16.5-0-0, Quirigua seems to have used approximately the Julian year computed from the formulae 19 years = 235 moons and 81 moons = 2392 days.

In the matter of eclipses very little has as yet been identified in the inscriptions. In the *Dresden Codex*, however, we find a fully developed lunar eclipse table giving a series of eclipse syzygies over a period of 33 years, and grouping the moons in sixes and fives so that the ends of the groups always reach these syzygies. We also find a prominent date in the context, 9.16.4-10-8, 12 Lamat 1 Muan. When we find this same date reached by the moon grouping on Stela M at Copan, and when we find the succeeding dates at Copan with moon groupings which would fit such a system of eclipse syzygies, we feel warranted in saying that the Maya, of the later inscriptions at least, were familiar with the general method of eclipse occurence, and that Stela M at Copan, 9.16.5-0-0, is the place where they first adopted a lunar eclipse year to replace their former 12-moon lunar year arrangement of moons.

Our information regarding Venus likewise comes largely from the *Dresden Codex*. We find there reference to four tables of the movements of Venus, the first, third and fourth given in full, but of the second only the zero date is given. From the beginning of the first to the end of the last table covers a period of 384 years. We learn here their method of constructing the tables, of making necessary corrections in them, and their probable connection with the Long Count of the inscriptions. The average length of a Venus synodic revolution is now computed at 583.920 days; the Maya computation was a little shorter than 583.935 days, but we do not know just how much more accurate it was. The inscriptions contain numerous Venus glyphs, but comparatively few of them could have represented heliacal risings of the planet, and our knowledge of the glyph variations is insufficient for us to positively select the proper ones without the aid of the *Dresden Codex*.

Both FÖRSTEMANN and WILLSON recognize Mars, Jupiter and Saturn tables in the *Dresden Codex*, but I have not been able to convince myself fully that they exist; if they do exist I do not know how to use them. »

With respect to the correlation of Maya and European dates, a topic which has been discussed many times in *Isis* (S. G. MORLEY, 1920, *Isis*, 3, 292-94 ; J. ERIC THOMPSON, 1927, *Isis*, 13, 564 ; HERBERT J. SPINDEN, 1930, *Isis*, 15, 483), Mr. TEEPLE concludes (p. 113) :

« I regard it as quite unsettled, and much work needs to be done on the inscriptions before we may reach a conclusion. If we could postulate an unbroken calendar from the inscriptions, and that 12 Kan 2 Pop was July 16, 1553, then I should consider the matter closed with the equation 11.16.0-0-0, 13 Ahau 8 Xul = November 3, 1539. I feel sure that no real progress can be made by assuming a correlation and then trying to force agreements out of the inscriptions. By that method almost any correlation can be made to look plausible, provided no one examines it too closely. The work must proceed from the other direction, assuming that we do not know equivalent Christian dates unless and until our accumulated knowledge from the inscriptions forces them on us. Tedious search for all glyphs and Mayan dates of a given form in the inscriptions, tabulation and analysis of the results—this is the only method, aside from direct revelation, which will ultimately produce a correlation inspiring general confidence. »

The author is a consulting chemist maintaining an office in New York City, but the study of Maya hieroglyphs and astronomy has been his main recreation for many years. His memoir is valuable not only because of the results he has obtained but even more perhaps because of the many problems which it is raising (see p. 111-13). It will certainly promote the study of a very difficult, tantalizing, and fascinating subject.

I shall be very glad to send a copy of it to any qualified scholar who would undertake to investigate it and to discuss it in *Isis*.

This review was already written when the news of the author's death reached me. The following information is extracted from the *Boston Evening Transcript* of March 24, 1931 :

« Dr. JOHN EDGAR TEEPLE, consulting chemist and a leading figure in the development of the American potash industry, is dead in New York at the age of fifty-seven years [on March 23rd]. Dr. TEEPLE, who made his home in Montclair, N. J., built up the American potash industry almost singlehanded in the years after the Armistice, when the German supply of potash, long eliminated from international markets, again flooded this country. It was at this time that Dr. TEEPLE took charge of the plant at Searles Lake, in the midst of a desert in Southern California. There was an abundance of potash in this drie dup lake, but engineers had been unable to separate it from the borax. Through Dr. TEEPLE's research and organization a modern plant was built and

the Searles Lake project became the only primary potash-producing firm in the country. For this achievement in this and other fields Dr. TEEPLE received the 1927 Perkin medal for distinguished work in applied chemistry. In the earlier part of his career he had engaged in the manufacture of such chemicals as nitric, hydrochloric and sulphuric acids and chlorine. He made important contributions to the distillation of hardwoods and conducted researches in the distillation of pine wood and extraction of resin. Dr. TEEPLE, born in Kempton, Ill., on Jan. 4, 1874, was graduated from Valparaiso College, Ind., in 1894. He received the degree of Ph. D. from Cornell in 1903, where he later taught for five years. In 1904 he went to New York to be director of the Industrial Laboratories, and later took up work as consulting chemist and chemical engineer. For many years he served as director and treasurer of the American Chemical Society. He was the author of a book entitled The industrial development of Searles Lake brines (1929) and of many contributions to scientific journals. »

GEORGE SARTON.

Edmund O. von Lippmann. — *Entstehung und Ausbreitung der Alchemie.* Zweiter Band. Ein Lese- und Nachschlage-Buch. VII+257 p. Berlin, SPRINGER, 1931 (Rm. 24).

The indefatigable historian of chemistry of Halle gave us in 1919 a history of alchemy down to the thirteenth century which has remained to this day the best general treatise on the subject. This volume is so fresh in my memory that it was quite a shock to me to discover that I had reviewed it in vol. 3 of *Isis* (p. 302-05) ; that is, since its publication no less than thirteen volumes of *Isis* have appeared !

Since 1919 so many studies have been devoted to the various phases of ancient and mediaeval alchemy that it became imperative to publish a supplementary volume. The arrangement is alphabetic, which makes it impossible and useless to analyze it. For example, I shall quote the five first articles of each of the letters A, M, S.

Abracadabra, Abraxas, Abû-l-Qâsim, abyssos, acotum acerrimum. Magie, magische Quadrate, magisterium, Mandäer, Mandel. Salmanas, Salmiak, Salomon, Salpeter, Salze im alten Babylonien. A good many articles begin with the word Alchemie or derivatives (p. 9-36).

The author hopes that scholars will read his book from cover to cover in spite of the alphabetic arrangement, but such a hope is bound to be disappointed. Nobody can be expected to read a dictionary (except with a very definite and exceptional purpose), even if he knows full well that his reading will be abundantly rewarded as would be the case for Dr. v. LIPPMANN's book. The alphabetic order, however useful

for practical needs, is in fact nothing but intellectual disorder, and this cannot but discourage and disgust the most resolute readers. One cannot jump from Manichäer, to MANILIUS, then to männlich-weiblich and to mappae clavicula without fatigue and without harm. Moreover without some guidance and some kind of synthesis it is impossible to store in one's mind such disparate information.

Dr. v. LIPPMANN's book will be very useful, and every owner of his first volume should obtain the second, but it will have to be used as a dictionary.

As to details my only criticims refer to the Arabic materials. It is a pity that the proofs were not more carefully read from the Arabic point of view. Sometimes letters are punctuated, sometimes not ; sometimes long vowels are indicated, sometimes not. In such matters it would be much better to leave out such orthographic niceties always, than to introduce them without consistency. A case in point is AL-DSCHÂHIZ wherein the two last consonants should be punctuated, and why are that Arabic word and a few others (but not all) quoted with the article and classified as if that article were a part of them ? On p. 77 Dû'l Nûn should be written DHÛ'L-NÛN, and the translation « Besitzer der Fische » is doubly incorrect ; it really means « He of the fish » (the relationship being uncertain). By the way, that is the very name given to the prophet JONAH in the Qur'ân and one would not say that JONAH owned the « fish » in question ; it was rather the other way round at least for three days ! The Egyptian ṣûfî referred to was probably called DHÛ-L-NÛN in JONAH's remembrance (my *Introduction*, vol. 1, 592). The fact that Dr. v. LIPPMANN is generally so trustworthy makes it the more necessary to warn his readers that his Arabic words should not be copied without scrutiny and that his Arabic information should be carefully checked.

GEORGE SARTON.

F. J. Cole. — *Early theories of sexual generation.* X+230 p., 12 pls. 9 figs. in text. Oxford, The Clarendon Press, 1930 ($ 6.00).

Only those who have attempted to evaluate critically the evolution of opinion on scientific ideas of wide significance will be in position to appreciate the scholarly effort which the author of this treatise has expended in its elaboration. The subject itself, so long wrapped in impenetrable mystery and so deeply involved in human lore and in philosophical speculations, perforce awaited the invention and some degree of perfection of the microscope before any objective basis for its analsyis was laid. Even after the impetus given to biology by this physical extension of the optical potentialities of the human eye, there

still remained unsolved the exact significance of the fusion of egg and sperm and their coordinated functions in establishing the characteristics of the organism brought into being by their mutual attraction, fusion, and later progress through a life cycle as a new unit of a new pattern. It was not until the early part of the present century that the clear outlines of the gene hypothesis emerged as the result of the discoveries of cytologists and geneticists such as FLEMMING, VAN BENEDEN, FOL, BOVERI, SUTTON, MENDEL, DE VRIES, and MORGAN.

The author's previous incursion into the field of the history of protozoology (see *Isis*, 9, 198) had brought him into contact with some of the main actors in this drama of discovery. His analysis of progress in comparative anatomy (see *Isis*, 3, 124) and his tracing the steps in the development and application of anatomical injections also afford his readers ground for anticipating a thoroughgoing and critical treatment of the subject in hand.

It is this critical analysis and the completeness with which the entire literature has been searched and compared which gives to this book its lasting scholarly value. Only the discovery of unknown and unsuspected historical sources could now justify a new treatise in the field. The bibliography is full and detailed and the documentation satisfactory with one exception, namely, that in citations of the views of authors and in quotations from them, the page references are not given.

The eight chapters deal with the earlier and later history of the discovery, illustration, and interpretations of spermatozoa, the emergence, development, and quietus of the doctrine of preformation, the origin and survival of the competing doctrine of epigenesis, the early theories of fertilization and development, and a retrospect.

In this retrospect Professor COLE so concisely states the function of the historian in science that quotation is imperative :

« The present work is an attempt to record the complete history, as far as it is known to the writer, of a scientific adventure—the Preformation Doctrine. Other matters dealt with are supplementary and subordinate to this purpose. To justify the epithet ' complete ' the historian must evaluate *all* the activities which were aroused and put in motion by the doctrine under consideration, and in particular he should not ignore the errors, repetitions, and vain philosophy which impede the progress of every scientific generalization. The path of science cannot be planned, but is ever tortuous and dendritic, breaking away into innumerable side-tracks, which terminate with monotonous and baffling regularity in the wilderness. When the purpose is at length attained it seems incredible that so much time and energy should have been required and expended to achieve so modest a result. But however alarming such a review may be, it is unprofitable to neglect the causes which have combined to limit the expansion of scientific investigation in the past. The history of science can be written in such a way as to convey a wholly false impression, not only of the difficulties of research,

but of the disastrous consequences which any failure to attack those difficulties by integrative methods invariably produces. »

The causes which operated to delay the solution of the problem of preformation by the early naturalists were varied : lack of rapid means of communication ; the widespread opinion that it was neither ethically obligatory, necessary, or even helpful to familiarize themselves with the works of their predecessors ; the resulting unceasing flow of repetition and of unverified and unverifiable speculation ; and that « fatal preoccupation with philosophy which prefers the science of words to the austerities of serious research. » All too much these causes led to the neglect of SWAMMERDAM's wise injunction, « We must not surmise or invent, but *discover*, what Nature does. »

The swamping effect of the Preformation Doctrine is realized when the surviving contributions in the field of generation by the early naturalists are summarized and compared with what was accomplished in the seventeenth and eighteenth centuries in the fields of comparative anatomy and physiology. Briefly these were the discovery (1) of spermatozoa, and (2) of the ova of the Vivipara, finally completed by VON BAER (1827), (3) the belief that a single spermatozoan entered the egg, but with no conception of the nature of fertilization, and (4) the theory of Epigenesis supported by WOLFF (1759) and established by VON BAER (1828). « The old epigenesis, however, applies only to descriptive embryology, and at best is but a first step towards a comprehension of the final causes of animal development. Ontogeny may yet find its explanation in preformation rather than in epigenesis. »

It is to be hoped that the author's title is prophetic and that this classic will be followed by a treatise on the Recent Theories of Sexual Generation.

CHARLES A. KOFOID.

Philip Ainsworth Means. — *Ancient civilizations of the Andes.* XVIII+ 586 p., 224 illus., map. New York, SCRIBNER's, 1931 ($ 7.50).

Though *Isis* is not a journal of archaeology and ethnology and any attempt to survey the immense literature devoted to these fields is obviously out of the question, yet our readers ought to be periodically reminded of their existence. The study of New World archaeology is of special interest because the civilizations which developed in Central America and in the Andean area were as independent from the Old World civilizations as any human culture could ever be from another. We have already stressed that point when reviewing CLARK WISSLER's books (*Isis*, 9, 138-42). We can emphasize it again using MEANS' own words (p. 535) :

« The basic constitutive elements of Old World culture include : cereals such as wheat, rye, barley, oats, and rice; draught- and burden-bearing animals such as reindeer, yaks, camels, dromedaries, elephants, horses, asses, mules, and dogs, food-yielding animals such as cattle, sheep, pigs, and poultry; foods derived from animals, such as milk, cheese, and eggs; metallic substances such as iron and its daughter, steel; mechanical devices involving one or more applications of the principle of the wheel, whether for the fashioning of vehicles or for the confection of tools varying greatly in kind and in elaboration; forms of building based upon the principle of the keystone arch, whether for simple arches or for domes, or for vaultings. These things, taken together, not only characterize Old World civilization, but define it. »

In the Americas the plants named above were unknown, instead of which the aborigines were acquainted with maize, potato, oca, squashes and thirty-six others ; for domestic animals they had only the llama, the vicuña and their kin (in the Andean area) and dogs ; eggs, milk, butter and cheese were totally unknown. The Incas had a name for iron (quillay) but had no more idea of its use and value than other Americans. They could use copper, bronze, silver, and gold very skilfully, but from a mechanical standpoint the only metal that matters is iron (and steel); thus in that respect it is no exaggeration to say that the Stone Age lasted in the New World at least until the end of the fifteenth century.

« Moreover, wheeled contrivances of every kind were universally unknown in ancient America, the nearest approaches to them, such as firedrills and spindles, being purely fortuitous and quite without any application of the wheel-principle. Finally, the lack of knowledge concerning the arch inevitably limited the scope of architects' imaginations to such edifices as the stepped-in vault could compass and, in ancient Peru, that was used but rarely, the roofs of early Andean structures being their least admirable feature. » (p. 536).

With regard to vaults, it should be noted that a curved vault has recently been discovered in a great circular tower at Caracol in the Maya area (1).

As opposed to the ancient civilizations of the Old World the creators of which have vanished or become unrecognizable, Andean and Mayan civilizations are still represented to this day not only by impressive monuments, but by living men and women. Says the author (p. 538) :

« The race which created even the oldest, the choicest, the rarest, and, for us, the most exotic artifacts from the ancient periods of the Andean area, is still living and, relatively speaking, is almost unchanged in custom, character, and genius. Years of observant wandering and diligent study have convinced me that of all the ancient and still-surviving peoples none is more tenacious of its

(1) A. V. KIDDER, Year Book of the Carnegie Institution of Washington (vol. 29, 100, 1930).

past, and none more likely—under favorable circumstances—to surprise the world some day with a splendid renascence of its former genius than is the venerable native stock which to-day comprises more than half of the Andean population. In spite of all the evils that have weighed upon them for four centuries their spirits are not crushed. »

The book is divided into twelve chapters, as follows : 1. Geographical background ; 2 to 7, History down to the Spanish conquest in 1531-32 ; 8 to 10, Incaic institutions and culture ; 11. The art of the loom in ancient Peru ; 12. Final points. There is an excellent bibliography ; the index is insufficient, and we miss a glossary of Indian terms, a great many of which are quoted and defined in the text. The chapter on Peruvian textiles (p. 450-523) is very elaborate ; it includes an account of the amazing feather-work applied upon cotton fabrics. There are 224 beautiful illustrations which are specially valuable for the sections devoted to the textiles and other arts.

To the historian of science this survey of Andean culture will be somewhat disappointing because he would expect a fuller and more systematic treatment of scientific subjects. However there is more in MEANS's book, e.g., on astronomy and medicine, than the defective index reveals. The means of land communications are well described (p. 332 sq.), and their relative perfection is very striking as compared with the backwardness of the methods of navigation. The perfection of the land communications in the immense Inca empire suggests that their geographical knowledge was developed ; there is a possibility that they used maps in relief, though this is not proved (p. 342). Inca astronomy is discussed in the chapter on ceremonial life and official religion. That knowledge was vastly inferior to that of the Mayas and of the Aztecs, but it was not contemptible. « Not only were they fully aware of the times when the solstices and the equinoxes occurred, but also they were able to adjust the lunar year, with its twelve complete cycles of phases, to the solar year by means of intercalating eleven supernumerary days. » (p. 367). The Echenique gold plaque studied by Sir CLEMENTS MARKHAM at Lima in 1853, and which had disappeared, is now in the Museum of the American Indian in New York City (p. 387, 389).

Information on *quipu* (2) may be found passim, by means of the index (see also p. 263, 333). A novelty for the reviewer was the note (p. 327-29, fig. 158) on *contadores*, but are these curious objects really counters ? The author inclines to believe that they were connected with the game of *chuncara*. With regard to medical lore and practice we are not told much (p. 63, 444-46). To be sure there is not very

(2) For quipu, see *Isis*, 6, 219; 8, 638, 810; 10, 292; 11, 548.

much to say, but a medical historian would have used the available materials to greater advantage. After all we owe to Peru two potent drugs, *cuca* and *chinchona* (cocaine and quinine). The Incas used the former, but it is uncertain whether they had discovered the latter. The account of Peruvian music is equally bare, but our readers will probably remember the excellent book on the subject by M. and Mme. D'HARCOURT (2 vols., Paris, 1925 ; *Isis*, 10, 524-26).

The author's synchronic table of the ancient American cultures deserves to be reproduced (table 1).

TABLE I

CENTRAL AMERICAN AND ANDEAN CULTURAL HISTORY COMPARED

(On a basis of native data recovered by modern research)

<table>
<tr><th></th><th>B. C.</th><th>—</th><th>100</th><th>0</th><th>100</th><th>200</th><th>300</th><th>400</th><th>500</th><th>600</th><th>700</th><th>800</th><th>900</th><th>1000</th><th>1100</th><th>1200</th><th>1300</th><th>1400</th><th>1500</th><th>A. D.</th></tr>
<tr><td>Central America</td><td colspan="2">Archaic and Introductory</td><td colspan="6">"Old Empire" of the Mayas and period of northwardly expansion into Yucatan</td><td colspan="3">First period of decline</td><td colspan="2">"New Empire"</td><td colspan="2">Toltec period</td><td colspan="2">Second decline</td></tr>
<tr><td rowspan="2">Andean Area</td><td rowspan="2" colspan="2">Archaic and Migratory</td><td colspan="5">In the highlands, Advanced Archaic or Tiahuanaco I period</td><td colspan="3" rowspan="2">Tiahuanaco II "empire" both on highland and coast</td><td>Decline</td><td colspan="3">Early Incas</td><td colspan="2" rowspan="2">Inca empire</td></tr>
<tr><td colspan="5">Early Chimu and Nazca cultures arising from archaic on the coast</td><td>Decline</td><td colspan="3">Late Chimu and Nazca on coast</td></tr>
</table>

That table is followed by a longer one (p. 48-49) by means of which the development of the « coast » and « mountain » cultures of the Andean area can easily be compared.

GEORGE SARTON.

Arturo Castiglioni. — *Histoire de la médecine*. Edition française établie par les soins de l'auteur. Traduction par J. BERTRAND, agrégée de l'Université et F. GIDON. Avec 279 gravures. 781 p. Paris, PAYOT, 1931 (120 fr.).

L'édition originale de ce bel ouvrage « Storia della medicina » (Milan, Unitas, 1927) date déjà de quatre ans, et je regrette vivement de n'avoir pu en rendre compte plus tôt dans *Isis* (voir cependant une courte note dans le vol. 13, 251). Ce retard n'est d'ailleurs pas dû à ma propre négligence, mais à celle des éditeurs italiens qui jugèrent inutile de m'en envoyer un exemplaire.

J'ai parcouru la traduction française avec plaisir, lisant quelques pages ça et là, et ma première impression est excellente. Le meilleur moyen de donner une idée générale d'un tel livre c'est d'examiner son architecture et d'en indiquer les proportions. Raconter l'histoire de la médecine en un seul volume est évidemment une sorte de gageure ; on ne peut tout dire, il s'en faut de beaucoup ; il faut donc choisir. Il est dès lors fort intéressant de savoir quels principes ont dirigé le choix de l'auteur.

Voyons comment la matière est partagée. (Les chiffres entre parenthèses indiquent le nombre de pages consacrées à chaque partie).

Introduction (9). Origines, médecine primitive (17). Mésopotamie (13). Egypte (15). Israël (14). Iran et Inde (13). Amérique pré-colombienne et Extrême-Orient (10).

Grèce (64). Période gréco-romaine (43).

Décadence et christianisme (15). Arabes (19). Moyen âge chrétien (93). Renaissance (72).

XVII^{e} siècle (66). XVIII^{e} siècle (73).

Première et deuxième moitiés du XIX^{e} siècle (80, 122).

Ou plus simplement :

Origines et médecine orientale (91). Antiquité classique (107). Moyen âge (127). Renaissance (72). XVII^{e} siècle (66). XVIII^{e} (73). XIX^{e} (202).

Il est curieux de comparer cet arrangement avec celui du livre anglais de Fielding H. Garrison (4^{e} éd., 996 p., 286 ill., Philadelphie, 1929) ; ce qui est assez facile si l'on se reporte à l'analyse que j'en ai faite dans *Isis* (vol. 13, 137-38). La différence essentielle peut se résumer ainsi : Garrison consacre au XIX^{e} siècle un peu plus de 40 % de son ouvrage, tandis que Castiglioni y consacre moins de 26 % du sien. Il s'en suit naturellement que le compte rendu de la médecine moderne que nous donne Garrison est beaucoup plus riche (d'autant plus que son livre est plus long que l'autre), tandis que celui de la médecine orientale, ancienne, et médiévale est beaucoup plus pauvre. Quelle méthode vaut le mieux ? On pourrait discuter là-dessus indéfiniment. Garrison nous donne d'excellentes raisons pour justifier la sienne : il nous dit que les étudiants en médecine et les médecins s'intéressent surtout à la médecine moderne et que c'est donc cette partie du sujet qui doit être traitée le plus largement. Sans doute, mais est-ce que le XIX^{e} siècle est si important que cela ? Compte-t-il à lui seul à peu près autant que tout le reste du passé ? Et est-ce faire œuvre de bon historien que de donner cette illusion aux médecins, ou plutôt de confirmer leurs défauts de perspective ? Je ne le pense pas, et tout compte fait, tout en reconnaissant les grands mérites du livre de Garrison, je préfère de beaucoup la méthode du professeur de Padoue.

Peut-être le lecteur sera-t-il tenté de ne voir dans cette opposition

de méthodes qu'un exemple de plus de l'opposition Amérique-Europe, mais je crois qu'il aurait tort. Quand les Américains s'intéressent au passé, ils s'y intéressent passionnément, et même dès qu'ils parviennent à se délivrer de ce qu'on pourrait appeler l'illusion ou la vanité moderniste, ils n'hésitent guère à se plonger dans le passé le plus lointain. Faut-il rappeler que c'est à un savant américain que nous devons l'admirable édition du Papyrus Smith? (*Isis*, 15, 355-67).

Pour en revenir au livre de CASTIGLIONI, son arrangement me parait fort bien équilibré, tant en gros qu'en détail. Les chapitres contiennent en général autant d'information qu'on peut s'attendre à y trouver étant donnée leur longueur, et chacun est suivi d'une bibliographie bien choisie. Bien entendu je préférerais pour le moyen âge une autre subdivision plus strictement chronologique : les efforts des Chinois, des Hindous, des Arabes, des Juifs, des Chrétiens ne se sont pas suivis ; ils ont été en grande partie simultanés. La division adoptée par l'auteur (qui n'a fait en cela qu'imiter les errements de ses prédécesseurs) ne permet pas d'apprécier suffisamment la simultanéité de ces efforts, leur parallélisme et parfois leur convergence.

L'idée de grouper en un seul chapitre la médecine américaine et la médecine chinoise me parait tout à fait grotesque. Quelle que soit la parenté ethnique de ces peuples, leurs idées médicales sont aussi divergentes que possible. Ce qui caractérise la médecine chinoise au dessus de tout, c'est l'excès de rationalisme (*Isis*, 14, 255-63) ; elle nous donne une idée de ce que la médecine européenne aurait pu devenir sous la direction de scolastiques tels que RAMON LULL, si l'influence de ceux-ci n'avait été tenue en échec par la persistance de l'esprit hippocratique et finalement détruite par le progrès de la méthode expérimentale.

Les médecins qui liront le livre de CASTIGLIONI auront une bonne idée de la médecine grecque et de la médecine gréco-romaine, même si les devoirs de leur profession ne leur permettent pas de lire davantage.

En particulier la médecine étrusque et romaine est fort bien traitée. On a si souvent répété que la médecine romaine n'était qu'un médiocre démarquage de la médecine grecque qu'il était vraiment utile d'en souligner les mérites ; il ne faut pas nous plaindre, mais plutôt nous réjouir de ce que le patriotisme de l'auteur l'ait conduit à écrire ce plaidoyer d'ailleurs fort modéré.

Le traitement de la médecine arabe me parait fort insuffisant : c'est certainement la partie la plus faible du livre, et j'espère que des éditions ultérieures donneront à l'auteur l'occasion de la remanier. A vrai dire aucune histoire générale de la médecine n'est satisfaisante à ce point de vue, mais on ose à peine blâmer les médecins de ne pas mieux comprendre l'Islâm quand la plupart des médiévalistes ne le comprennent guère

mieux. Je me suis étendu si longuement là-dessus dans mes propres écrits qu'il n'est pas nécessaire d'y insister ici.

Les opinions de l'auteur sont en général d'une grande modération. Voir par exemple sa discussion de l'origine de la syphilis. D'autre part sa nationalité italienne introduit quelques points de vue nouveaux : j'y ai déjà fait allusion à propos de la médecine romaine ; un autre exemple est sa discussion de la découverte de la circulation du sang.

Les notes bibliographiques assez nombreuses contiennent beaucoup plus de travaux italiens que n'en mentionnent les livres allemands et anglais. Tant mieux, car la plupart méritent d'être mieux connus. Je recommande donc vivement aux historiens anglais et américains de consulter ce livre : ils s'étonneront parfois de n'y pas trouver des indications qui leur sont familières, mais d'autre part ils en trouveront d'autres qui leur sont restées jusqu'ici inconnues. Les illustrations sont abondantes et souvent originales. A ce point de vue encore les ouvrages de GARRISON et de CASTIGLIONI se complètent fort bien.

L'auteur habite à Trieste et est professeur d'histoire de la médecine à l'Université de Padoue. Il a écrit un grand nombre d'articles historiques dont beaucoup ont été signalés dans *Isis* (voir vol. 4 et suivants). Ses principaux ouvrages et mémoires sont :

La vita e l'opera di SANTORIO CAPODISTRIANO (L. CAPPELLI, Bologne, 1920 ; *Isis*, 4, 412) ; UGO BENZI da Siena e il Trattato utilissimo circa la conservazione della sanitade (Sienne, 1921 ; *Isis*, 5, 504) ; La farmacia italiana del' 400 nella storia dell' arte ceramica (F. LEGA, Faenza, 1922) ; Il Libro della pestilenza di GIOVANNI DE ALBERTIS da Capodistria (L. CAPPELLI, Bologne, 1924 ; *Isis*, 7, 539) ; Il Volto di IPPOCRATE. Istorie di medici e medicine di altri tempi (Società Editrice Unitas, Milan, 1925) ; Storia dell'igiene, Trattato italiano d'Igiene. (Unione Tipogr. Editrice, Turin, 1926) ; Storia della medicina (Società Editrice Unitas, Milan, 1927) ; Storia della tubercolosi (F. VALLARDI, Milan, 1930).

GEORGE SARTON.

Sir D'Arcy Power. — *Selected writings*, 1877-1930. x+368 p., 16 pls., 1 map, 9 figs. in text. Oxford. At the Clarendon Press. 1931 ($ 8.50).

This collection of essays in the history of medicine was conceived by members of the Osler Club of which Sir D'ARCY POWER was elected a « friend », and carried out by a committee of eminent physicians for presentation to the author of the writings on his seventy-fifth birthday. The task of selection of appropriate subjects from among the six hundred and nine scientific and clinical articles, essays in the field of medical history and biography, and contributions to encyclopedias and dictionaries

will be appreciated by those who scan this classified list of titles which is appended to the selections. The wide range of the field covered between his first article on albuminaria in 1877 to his presidential address before the Section of Comparative Medicine of the Royal Society of Medicine published in 1927, and the historical perspective in which his writings were ensconced, can be appreciated from the titles listed and those honored in this selection. His first essay into the field of medical history was in the editing of SOUTH's *Memorial of the Craft of Surgery*. This was followed by a long series of unsigned historical articles in the British Medical Journal. No less than one hundred and eighty-four biographies from his pen are included in the Dictionary of National Biography. The keen historical sense which he had and the analytical skill in setting forth the data he turned up in his manifestly wide reading is nowhere better shown than in his *Education of a Surgeon under Thomas Vicary* delivered before the Royal College of Surgeons in 1920, in the *Imaginary Annals* in the address before the Section on Comparative Medicine alluded to above, and in that on *How the tradition of British Surgery came to America* before the American Surgical Association at Baltimore in 1924. The essay on *Why Samuel Pepys discontinued his Diary* requires not only a familiarity with this famous diary, but also a knowledge of the literature of optics as applied to the making of spectacles from ROGER BACON to CHEVALIER. The evidence indicates that the diarist had a hypermetropic astigmatism which could not have been cured by the glasses available in his lifetime.

Among the other titles included in the sixteen selections in this work are *John Hunter, a Martyr to Science*, in which the evidence is marshalled to show that JOHN HUNTER died not from *angina pectoris*, as stated by Sir EVERARD HOME, but rather of syphilis. In 1767 HUNTER inoculated himself with pus from a patient with gonorrhoea, in order to determine whether or not this disease was identical with syphilis. The history of the experiment and the later history of the experimenter clearly show in the light of modern knowledge of venereal diseases that this zeal for knowledge not only resulted in his later illnesses, but ultimately cost him his life. This episode in the career of the founder of the Hunterian tradition is significant of the atmosphere of scientific inquiry created by him and transmitted to his followers.

The book with its very ample index is a mine of medical lore, a collection of superb examples of British scientific historical writing, and a most enjoyable volume to read.

CHARLES A. KOFOID.

M. E. M. Walker. — *Pioneers of Public Health.* The story of some

benefactors of the human race. With a foreword by Sir HUMPHRY ROLLESTON. XVI+270 p., 23 pls. New York, MACMILLAN Co. 1930. ($ 4.50).

The authoress of this « tribute to the bright memory of my only son who died in the tropics » has compiled biographies of twenty-one leaders in the field of hygiene, preventive and exotic medicine. Of the leaders selected for inclusion twelve are British, four are from the United States, three are of Germanic origin, and two are from France. The bacteriologists naturally take a prominent place and include PASTEUR who founded the science, KOCH who developed its relations to the greatest of human plagues, and LISTER who established its lasting significance to surgery. The protozoologists and parasitologists have an even larger numerical representation, including TIMOTHY LEWIS who noted the organisms in the faeces of victims of cholera, LAVERAN who discovered the malarial parasite in the human blood, MANSON who found Microfilaria in the mosquito and thus laid the foundation for the later control of disease by elimination or avoidance of insect vectors, and LEISHMAN for whom a group of blood parasites are named. Epidemiology is represented by THOMAS SYDENHAM, the English Hippocrates whose views on this subject have of late been revived, by General W. C. GORGAS who applied the principles of this science on a grand scale in the Canal Zone and whose trained personnel now serves the extensive and successful medical service of the United Fruit Company in the American tropics, and by Dr. WALTER REED whose sacrifice established the basis of the scientific knowledge of yellow fever.

Hygiene is represented by PETTENKOFER who made the subject respected in academic circles, by LIND the naval, and by PRINGLE the military hygienist. Public health and its administration are introduced by FRANK, a « modern of the moderns, » CHADWICK, SIMON, and PARKES who fostered public interest and enterprise in this field in Great Britain, and by HERMANN BIGGS who demonstrated in the United States the value of this work in the greatest metropolis of America. Preventive medicine is represented by many, if not all, of the names listed above, but preeminently by JENNER the introducer of vaccination. The significance of vital statistics was persistently advocated by the layman LEMUEL SHATTUCK in Massachusetts, and by WILLIAM FARR in Great Britain.

It is doubtless impossible to select candidates for such biographical distinction without omitting many whose claims for representation are great. Nevertheless, the omission of WILLIAM T. SEDGWICK the American sanitarian who linked engineering practice to public health interests in the United States and elsewhere, is inexcusable. To a like degree the work of Sir RONALD ROSS on malaria has a permanent importance

far exceeding that of not a few of those included in this Valhalla of the benefactors of the human race.

CHARLES A. KOFOID.

Richmond Cranston Holcomb. — *A century with Norfolk Naval Hospital* 1830-1930. 543 pp., 42 ills. Portsmouth, Virginia Printcraft Publ. Co. 1930.

Hospitals in their history reflect progress in the medical sciences, though some phases of it are often somewhat belated in appearance. On the whole, however, they exhibit the more permanent contributions to medical progress which have stood the tests of practical utility, effectiveness, and success elsewhere. In its growth a naval hospital adds to these scientific factors those which arise from the internal growth of the navy itself, in its changing marine, mechanical, and engineering developments, and from both internal and external political influences. Most of all its expansions follow upon the catastrophe of war.

The Norfolk Naval Hospital is the main hospital of the United States Navy and its history is bound up with that of the Medical Department of this arm of the government. Naval surgeons were provided in the U. S. Navy when it was established by Congress in 1776 with ratings and pay proportioned to the number of guns carried by the ship. Examining boards appointed by the several states were recommended by Congress in 1777 and regulations providing for the care of sick and wounded were set up. At the close of the Revolution the ships of the navy were all sold, and a Navy Department was not created until 1798, when medical officers were again commissioned as surgeons and surgeon's mates. The first of these surgeons was Dr. GEORGE BALFOUR and he took charge of the first marine hospital in Norfolk County. Va. A Medical Department for both army and navy was created in 1799 and the first appropriation was made for equipment for care of the sick.

The sick were located on the main deck between two gun posts, hence this space came to be known as the « sick bay ». Limes and lemons were used in 1800 to ward off scurvy. Navy regulations prescribing the duties of medical officers were established in 1818. The surgeon's mate visited, bled, dressed wounds, gave medicines with his own hands, put up prescriptions, kept the cock-pit and instruments clean, inspected the coppers, and gave out liquors to his aid the loblolly boy for distribution. Battle surgery was carried on in the heat and darkness of the cockpit below the waterline. Vinegar was used as an antiseptic, rum and laudanum for deadening of pain, and straps for restraint.

In 1855 a yellow fever epidemic followed the arrival of a steamship from St. Thomas in the West Indies at Norfolk which reported two

deaths en route and buried a sailor on arrival. Quarantine followed but in the absence of knowledge of the agency of Stegomyia, the yellow fever mosquito, it was futile, and the disease quickly spread into the city. In three months six hundred cases were treated in the hospital with a mortality of thirty-five per cent. In the light of the evidence investigators of the epidemic reasoned that yellow fever was not contagious, a conclusion borne out later on more scientific grounds. But they also argued that yellow fever would have visited Norfolk even if the ship « Benjamin Franklin », which arrived from the West Indies, had never visited the plague stricken Norfolk. The grounds for this argument were mainly that this dread disease « never yet has made its advent remote from wharves, docks, or low marshy and new made grounds, except on shipboard. » There was no suspicion in these arguments that the mosquito guilty of this epidemic was breeding in the bilge of the Franklin upon her arrival. It needed the historical background of the work of MANSON, ROSS, GRASSI, FINDLAY, WALTER REED, LAZEAR, AGRAMONTE, and NOGUCHI to illumine this dark problem.

Dr. HOLCOMB's history is mainly concerned with local and naval interests. The comparison of early conditions with the modern equipment and practice shows how the discoveries of the collateral sciences, often cited by the author, have improved, diversified, extended and rationalized the functions of the hospital.

CHARLES A. KOFOID.

Thirty-First Critical Bibliography

of the

History and Philosophy of Science and of the History of Civilization

(to March 1931)

This thirty-first Bibliography contains about 650 items, some of which have been kindly contributed by the following scholars :

C. ADAMS (Hertford, England).
C. A. KOFOID (Berkeley, Cal.).
C. D. LEAKE (San Francisco, Cal.).
Y. MIKAMI (Tokyo).
J. PELSENEER (Cambridge, Mass.).
A. POGO (Cambridge, Mass.).

It includes the thirteenth series of addenda and errata to Volume I of my *Introduction to the History of Science* (Baltimore and London, 1927). An author's index will be found at the end.

Papers and books sent to the Editor are entered into this bibliography if they are relevant to its purpose. They may be the object of analytical notes or longer reviews, if this seems sufficiently necessary and the services of competent scholars are available. It is obvious that only a small number of publications can be adequately reviewed and that it is not even possible to give a brief description of each and every one. However, our classification is so comprehensive and so minute, that even if we do nothing but give the accurate title of an item and put it in the proper place, we are already rendering a real service to the author, the publisher, and the learned public. There is no relation between the importance of a publication and the length of the note devoted to it.

The ultimate aims of this undertaking are : (1) to establish the History of Science as an independent discipline ; (2) to demonstrate inductively the Unity of Knowledge and the Unity of Mankind ; (3) to serve as a center of information and rallying ground to the scholars engaged in our studies.

Harvard Library, Room 185.
Cambridge, Massachusetts.

GEORGE SARTON.
April 6, 1931.

PART I

FUNDAMENTAL CLASSIFICATION (CENTURIAL)

IXth Century B.C.

Körner, Otto. Die homerische Tierwelt. 2., f. Zoologen u. Philologen neubearb. u. erg. Aufl. IV+100 p. München, BERGMANN, 1930. ISIS

Reviewed by AUGUST STEIER, *Deutsche Literaturzeitung*, 1, 2365-67, 1930; by H. KRAEMER, *Archiv f. Geschichte der Mathematik, der Naturwissenschaften und der Technik*, 13, 214-15, 1930.

Vth Century B.C.

Wellmann, Max. Die ps. hippokratische Schrift *Περὶ ἀρχαίης ἰητρικῆς*. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 299-305, 1930. ISIS

IVth Century B.C. (second half)

Lulofs, H. J. ARISTOTELES over uiterlijk en innerlijk. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 261-70, 1930. ISIS

Menetrier, M. P. Comment ARISTOTE et les anciens médecins Hippocratiques ont-ils pu prendre connaissance de l'anatomie humaine? *Bulletin de la société française d'histoire de la médecine*, 24, 254-62, 1930. ISIS

Sarton, George. ARISTOTLE and PHYLLIS. *Isis*, 14, 8-19, 5 fig., 1930. ISIS

History of that legend with iconographical and bibliographical notes, and four plates.

Stiebitz, Ferdinand. Über die Kausalerklärung der Vererbung bei ARISTOTELES. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 332-45, 1930. ISIS

IIIrd Century B.C. (second half)

Agostini, Amedeo. Notizie sul ricupero dei Libri V, VI, VII delle « Coniche » di APOLLONIO. *Periodico di Matematiche*, 8 p., 1930. ISIS

Stein, W. Der Begriff des Schwerpunktes bei ARCHIMEDES. *Quellen und Studien zur Geschichte der Mathematik.* Abteilung B : Studien, 1, 221-44, 5 fig., 1930. ISIS

IInd Century B.C. (second half)

Bévenot, Hugo. Die beiden Makkabäerbücher. Übersetzt und erklärt. XII+260 p. (Die Heilige Schrift des Alten Testamentes. Übersetzt und erklärt in Verbindung mit Fachgelehrten, herausgegeben von FRANZ FELDMANN und HEINR. HERKENNE, IV, 4). Bonn, PETER HANSTEIN, 1931. (Broschiert Mk. 9.60, gebunden, Mk. 11.60). ISIS

German translation based upon a critical study of the text, together with abundant notes, by our learned collaborator, Dom BÉVENOT, of the Benedictine abbey of Weingarten in Württemberg. The author has added two very clear maps and an excellent chronological table for the period 190 to 134. There are four books of MACCABEES, of which the first two are included in the Vulgate and the two others in the Septuagint (A fifth one is simply an Arabic compilation partly derived from JOSEPHUS' Jewish War). The first two are parts of the Catholic canon since the Council of Trent, 1546. From the Protestant point of view all are apocryphal. Dom BÉVENOT's edition is restricted to the canonical books, which are by far the most important : they constitute our best source for the most glorious period of Jewish history. These two books were probably composed in the last quarter of the second century B.C., or in the second half; the first was originally written in Hebrew, but is preserved only in Greek; the second was originally written in Greek being a summary of a larger (lost) work by JASON OF CYRENE. G. S.

Ist Century B.C. (whole and first half)

Beckmann, Franz. Geographie und Ethnographie in CAESARS Bellum Gallicum. 189 p. Dortmund, RUHFUS, 1930. ISIS

Heron of Alexandria (*Introduction*, vol. 1, 209). The English translation of the Pneumatics (London, 1851) was made by J. G. GREENWOOD. BENNET WOODCROFT was only the editor though his name alone appears on the title page. CH. ADAMS. ISIS

Ist Century B.C. (second half)

Rouch, J. La prévision du temps dans VIRGILE. *Revue générale des sciences*, 42, 19-25, 1931. ISIS

Ist Century (second half)

Josephus Flavius. Œuvres complètes de FLAVIUS JOSÈPHE. Traduites

en français sous la direction de Théodore Reinach. T. iv. Antiquités judaïques, i. XVI-XX. Traduction de G. Mathieu et L. Herrmann, avec le concours de S. Reinach et J. Weill. 301 p. Paris, Leroux, 1929. ISIS

Reviewed by Maurice Goguel, *Revue de l'histoire des religions*, 100, 257-58, 1929; by F. Perles, *OLZ*, 33, 38, 1930.

IInd Century (whole and first half)

Robbins, Frank Egleston. P. Mich. 620 : A series of arithmetical problems. *Classical Philology*, 24, 321-29, 1929. ISIS

« The papyrus which is numbered 620 in the collection of the University of Michigan should be of no small interest to the historian of mathematics, for it appears to be a fragment of a collection of arithmetical problems, written prior to the time of Diophantus, in which a quasi-algebraic method of solution is employed and in which appear some of the symbols used in the manuscripts of Diophantus, notably the sign ς, which denotes the unknown term. It is not known from what part of Egypt P. Mich. 620 came, but it is undoubtedly Egyptian, and was one of the purchases made in 1921 by the late Professor Francis W. Kelsey. Paleographically it seems to belong early in the second century after Christ; perhaps it may be even earlier. The writing is of the literary type but rather irregular. There is no indication in the papyrus who its author may have been. »

IInd Century (second half)

Dyroff, Adolf. Über die Abhängigkeit medizinischer Richtungen von der Philosophie, gezeigt an Galen. *Proteus*, 1, 9-12, Bonn, 1931. ISIS

IIIrd Century (whole and first half)

Irsay, Stephen D'. Christian medicine and science in the third century. *Journal of Religion*, 10, 515-44, 1930. ISIS

IVth Century (second half)

Bakker, C. Enkele fragmenten uit de werken van Oribasius, waaruit de waardeloosheid der signatuurleer blijkt. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 285-93, 1930. ISIS

Vth Century (whole and first half)

Stegemann, Victor. Astrologie und Universalgeschichte. Studien und Interpretationen zu den Dionysiaka des Nonnos von Panopolis.

VIII+257 p., 1 chart. (*Στοιχεῖα, Stud. z. Gesch. d. antiken Weltbildes u. d. griech. Wissensch.* Begr. v. FRANZ BOLL. H. IX). Leipzig, TEUBNER, 1930. ISIS

Reviewed by PAUL MAAS, *Deutsche Literaturzeitung* 1, 1890-94, 1930; by JULIUS RUSKA, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 20, 1931.

VIth Century (whole and first half)

Jundîshâpûr (*Introduction*, 1, 435). My localization of that city is incorrect, and contradictory. It is not anywhere near Kâzirûn, which is in the Shâpûr district in Fârs, between the old Shâpûr and Shîrâz. To say that it is between Susa (Sûs) and Ectabana (Hamadhân) is neither right nor clear. Jundîshâpûr was not very far from Sûs. Its site is marked by the ruins now called Shâh-âbâd, in N W Khûzistân, on the road from Tustar to Dizfûl, about eight leagues NW of Tustar. See G. LE STRANGE : Lands of the Eastern Caliphate (1905, 238) ; CL. HUART : Encyclopaedia of Islâm (vol. 1, 1064, 1913) ; E. G. BROWNE : Arabian medicine (1921, 19). My attention to this error was kindly called by HUGH P. VOWLES, of London, who sent me a long list of contradictory statements by various authorities on the subject (see *Nature*, 126, 993, 1930). The contradictions are due to the fact that the great Sâsânian king SHÂPÛR founded or rebuilt or attached his name to at least three cities : Jundîshâpûr in Khûzistân, Shâpûr in Fârs, and Nîshâpûr in Khurâsân. G. S. ISIS

Longman, Lester D. Two fragments of an early textile in the Museo Cristiano. *The Art Bulletin*, 12, 115-30, 22 fig., 1930. ISIS

VIIth Century (second half)

Fuchs, Walter. Zwei neue Exemplare des mandjurischen Kandjur. *OLZ*, 33, 970-72, 1930. ISIS

Grousset, René. Sur les traces DU BOUDDHA. IV+329 p., 4 pl. Paris, PLON, 1929. ISIS

Referring to the times of HSÜAN TSANG and I-CHING, and to the activities of these very men. G. S.

VIIIth Century (whole and first half)

Jones, Putnam Fennell. A concordance to the Historia Ecclesiastica

of BEDE. Publ. for the Concordance Soc. by the Mediaeval Acad. of America. IX+585 p. *(The Mediaeval Acad. of America. Publication no. 2.)* Cambridge, Mass., 1929. ISIS

Reviewed by WILHELM LEVISON, *Deutsche Literaturzeitung*, 1, 2246-47, 1930; by JOHN S. P. TATLOCK, *Speculum*, 150, 1931.

IXth Century (whole and first half)

Woodruff, Helen. The physiologus of Bern. A survival of Alexandrian style in a ninth century manuscripts. *The Art Bulletin*, 12, 226-53, 43 fig., 1930. ISIS

« The Stadtbibliothek at Bern possesses several manuscript copies of the Physiologus, a writing which attempts to explain the appearance and habits of animals, both real and fabulous, of a few stones, and some plants, and which in the years after its compilation was expanded by exegetic writing to include significant allegories or parallels from the Bible. One of these manuscripts is illuminated. It is hoped to show in the following discussion that this manuscript, Codex 318, dating from the ninth century, and the oldest copy known at the present time which is illustrated, is of considerable importance, not only stylistically and iconographically, but also with regard to the history of the Physiologus itself. »

IXth Century (second half)

Bar 'Alî (Isho). The Syriac-Arabic glosses. Edited by RICHARD J. H. GOTTHEIL. p. 237-496. *(Memorie della R. Academia Nazionale dei Lincei, Classe di Scienze Morali, Storiche e Filologiche*, serie 5, vol. 13, parte 2). 1910-1928. ISIS

Reviewed by C. BROCKELMANN, *OLZ*, 33, 364-67, 1930.

Meyerhof, Max. The « Book of Treasure », an early Arabic treatise on medicine. *Isis*, 14, 55-76, 1930. ISIS

Analysis of the Kitâb al-dhakîra fî 'ilm al-ṭibb, a medical encyclopaedia ascribed to THÂBIT IBN QURRA. Glossary. G. S.

Asín Palacios, Miguel. El « Libro de Los Animales » de JÂḤIẒ. *Isis*, 14, 20-54, 1930. ISIS

Analysis of the Kitâb al-ḥayawân of AL-JÂḤIẒ and extracts translated into Spanish treating of fire in mines (if the fires goes out in a pit, man cannot live in it), animal mimetism, struggle for existence, ants, effects of castration in man. G. S.

Xth Century (whole and first half)

Bernfeld, Werner. Eine Beschwörung der Gebärmutter aus dem frühen Mittelalter. *Kyklos*, 2, 272-74, 1929. ISIS

Macler, Frédéric. Anania Mokatsi écrivain arménien du X^e siècle. *Revue de l'histoire des religions*, 101, 5-15, 1930. ISIS

« Les quelques exemples proposés suffisent à établir et à prouver l'importance de l'œuvre d'Anania Mokatsi. Cette œuvre apporte des précisions et des renseignements sur une période capitale et dominante de l'histoire d'Arménie, l'époque brillante des Bagratides, des Siwniq et des Ardzrounis. Elle nous aide à connaître surtout un moment troublé de l'histoire des Aghouanq, des Arméniens et des Géorgiens, où, à la suite de querelles religieuses, ces nations du Sud, de l'Ouest et du Nord-Est du Caucase allaient suivre une politique différente et parfois ennemie, alors qu'au début de leur conversion au christianisme — et ce jusqu'au Concile de Chalcédoine — ces peuples vivaient unis, soumis à la même destinée politique et religieuse. »

Xth Century (second half)

Ashdown, Margaret. English and Norse documents relating to the reign of Ethelred the Unready (978-1016). Cambridge University Press, 1930. ISIS

The nucleus of this volume is an edition of the Old English *Battle of Maldon*, and an attempt to estimate the literary and historical value of the poem. The volume further aims at showing, through the use of material for a limited period, how Norse literature may throw light on English History. The material presented also suggests a comparison between English and Norse methods of historical writing, in prose and in verse.

XIth Century (whole and first half)

Aḥmad, Maqbûl. A Persian translation of the eleventh century Arabic alchemical treatise 'Ain aṣ-ṣan'ah wa 'aun aṣ-ṣana'ah. To which is annexed a note on the chemistry of the processes given in the treatise, by B. B. Datta. (*Memoirs of the Asiatic Society*, 8, 419-60). Calcutta, 1929. ISIS

Reviewed by George Sarton, *Isis*, 14, 229-32, 1930.

(Firdawsî). The Shû-nâmah of Firdausi. With 24 illustrations from a fifteenth century manuscript formerly in the Imperial Library, Delhi, and now in the possession of the Royal Asiatic Society. Described by J. V. S. Wilkinson with an introduction on the paintings by Laurence Binyon. 92 p., 24 pl. (some of which in color). The India Society, 3 Victoria St., London, S. W. 1, 1931. ISIS

Gray, Edward. Leif Eriksson. Discoverer of America, A.D. 1003. xxxii+188 p. Oxford, University Press, 1930. ISIS

Guest, A. R. Further Arabic inscriptions on textiles (III). *Journal of the Royal Asiatic Society*, 761-66, pl., 1930. ISIS

Hauer, J. W. Das neugefundene arabische Manuskript von AL-BÎRÛNÎ Übersetzung des Pâtañjala. *OLZ*, 33, 273-82, 1930. ISIS

Ibn Janâh. Sefer hariqma (Kitâb al-luma'), ab JONA IBN GANÂḤ lingua Arabica conscriptus. Hebraice vertit JEHUDA IBN TIBBON. Ad quattuor versionis codicum fidem collato ipso auctoris libro edidit praefatione notis indicibus auxit MICHAEL WILENSKY. Fasc. I, 208 p., 4 pl. *(Scripta Academiae Literarum Judaicarum : Corpus grammaticorum et exegetarum Judaeorum)*. Berlin, Akademie-Verlag. 1928. ISIS

Reviewed by W. WINDFUHR, *OLZ*, 33, 200-01, 1930; by H. TORCZYNER, *DLZ*, 1, 2363-65, 1930.

Lippmann, Edmund O. von. Ein neues arabisches Manuskript über Alchemie aus der Zeit um 1000. *Proteus*, 1, 31-35, Bonn, 1931. ISIS

Apropos of a treatise dating from c. 1030 (1034) by ABÛ-L-ḤAKÎM MUḤAMMAD IBN 'ABD AL-MALIK (see my *Introduction*, vol. 1, 723; *Isis*, 14, 229-32). G. S.

Mackinney, Loren C. Bishop FULBERT : teacher, administrator, humanist. *Isis*, 14, 285-300, 1930. ISIS

Thórdarson, Matthias. The Vinland voyages. Translated by THORSTINA JACKSON WALTERS. With an introduction by VILHJÁLMUR STEFÁNSSON. 90 p., 20 photos. 3 maps. *(American Geographical Society, Research series no. 18)*. New York, American Geographical Society, 1930. ISIS

In 1930 there was celebrated at Reykjavik the millenial of the founding of the Parliament of Iceland. In recognition of that event the Society has just published a book on the Vinland voyages by MATTHIAS THORDARSON, the Director of the National Museum of Iceland. Mrs. THORSTINA JACKSON WALTERS has translated the text from the original Icelandic, VILHJALMUR STEFANSSON has written a most interesting introduction, and Professor HALLDOR HERMANNSSON, Curator of the Fiske Icelandic Collection at Cornell, has edited the book. Three maps convey an idea of the most probable routes of the first discoverers of the mainland of North America. There are photographs of Iceland, Greenland, Labrador, and the St. Lawrence region to show the kind of country which the sagas describe. In broad strokes STEFANSSON sketches the background of Norse voyaging which preceded 1000 A.D., the boiling over again and again of « the caldron of Norse energy » which led to the colonization of Greenland and finally to the discovery of the North American mainland. THORDARSON's text includes an analysis of the saga records in terms of the geography of the northeastern coast of the continent. A short account, easily read, the book is a contribution of modern scholarship to an absorbing historical problem.

'Ubaidallâh ibn Jibrîl ibn Bakhtyashû' (*Introduction*, vol. 1, 730).

I ought to have mentioned his bestiary, Manâfi' al-ḥayawân (or, Kitāb al-khawāṣṣ mujarrab al-manāfi') of which there are very interesting illustrated MSS., e.g., in the Bibliothèque nationale in Paris and in the Pierpont Morgan Library in New York. C. BROCKELMANN : *Arabische Litteratur* (vol. 1, 236, 483, 1898). F. S. BODENHEIMER : Geschichte der Entomologie (vol. 1, fig. 40-44, 1928; *Isis*, 13, 388). G. S. ISIS

Wickersheimer, Ernest. Note sur les œuvres médicales d'ALPHANE, archevêque de Salerne. *Janus*, 34, 273-78, 1930. ISIS

Il y eut à Salerne deux médecins nommés ALPHANE. « ALPHANE Ier (ou SAINT ALPHANE), issu de sang noble, avait été moine au Mont-Cassin, puis abbé de Saint-Benoît à Salerne, dont il occupa avec éclat le siège métropolitain à partir de 1058; il mourut en 1085, succombant peut-être à la maladie d'estomac dont CONSTANTIN L'AFRICAIN n'avait pas su le guérir. ALPHANE II lui succéda et mourut en 1127. L'un et l'autre ont laissé des écrits sacrés ou profanes, mais le bagage littéraire du second est bien moins considérable et, comme d'autre part, on sait avec certitude qu'ALPHANE Ier fut médecin, il est tout naturel de lui attribuer les ouvrages médicaux qui portent le nom d'Alphanus archiepiscopus' ». Deux écrits d'ALPHANE ont été édités; le *Premnon physicon*, par C. BURKHARD (Leipzig, 1917) et le *De quattuor humoribus corporis humani* par PIETRO CAPPARONI (Rome, 1928, 27 p., pl.). L'auteur s'occupe surtout d'un autre traité *De pulsibus* (Ms. 1024 de l'Arsenal, Paris) encore inédit, dont il nous donne un résumé avec extraits. Il commence ainsi : « Tractatus archiepiscopi Alphani de pulsibus. Omnium signorum, corporis interiorem disposicionem declarancium, duo potissima inveniuntur, pulsus scilicet et urina. » G. S.

XIth Century (second half)

Hurd-Mead, Kate Campbell. TROTULA. *Isis*, 14, 349-67, 1930. ISIS

« From this brief review we have seen that the evidence of the manuscripts and of the printed books of each century has proved that TROTULA was a woman doctor of Salerno of the eleventh century who wrote a famous gynecology which was used for hundreds of years as a text book and work of reference by students and doctors. »

Meyerhof, Max. Über einige Privatbibliotheken im fatimidischen Aegypten. *Rivista degli studi orientali*, 12, 286-90, 1930. ISIS

« OLGA PINTO bemerkt in ihrer fleissigen Studie über arabische Bibliotheken : 'Delle biblioteche private dell'Egitto si hanno purtroppo scarse notizie... esistendo al Cairo una delle piu grandi e meravigliose biblioteche pubbliche, i privati sentirono poco il bisogno di averne delle proprie'. Das ist nicht ganz richtig; denn gerade aus dem V./XI. Jahrhundert, aus der Zeit des Bestehens dieser grossen, vom Chalifen AL-ḤĀKIM gegründeten Akademie-Bibliothek Dār al-'Ilm ('Haus der Wissenschaft') haben wir

Nachrichten über drei Privatbibliotheken. IBN ABI UṢAIBI'A berichtet darüber an mehreren Stellen in seiner grossen Geschichte der griechischen und arabischen Ärzte, and diese Stellen sind auch schon früher ausgezogen worden, aber wohl wieder in Vergessenheit geraten. Ich gebe sie deshalb nochmals in Übersetzung ».

Sudhoff, Karl. KONSTANTIN DER AFRIKANER und die Medizinschule von Salerno. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 293-98, 2 fig., 1930. ISIS

XIIth Century (whole and first half)

(Angkor Vat). Le temple d'Angkor Vat. Deuxième partie : La sculpture ornementale du temple. *(Mémoires archéologiques publiés par l'Ecole Française d'Extrême-Orient)* Paris, VAN OEST, 1931. ISIS

Conant, Kenneth John. Mediaeval Academy excavations at Cluny. *Speculum*, 3-14, 1 fig., 5 pl., 1931. ISIS

Corner, George W. The rise of medicine at Salerno in the twelfth century. *Annals of Medical History*, 3, 1-16, 1931. ISIS

Grassus, Benevenutus, of Jerusalem. De oculis eorumque egritudinibus et curis. Translated with notes and illustrations from the first printed edition, Ferrara, 1474 A.D. by CASEY A. WOOD. XIII+ 101 p., 5 pl. Stanford University Press, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 232-33, 1930.

Hammer, Jacob. Some Leonine summaries of GEOFFREY OF MONMOUTH's Historia regum Britanniae and other poems. *Speculum*, 114-23, 1931. ISIS

Mohammed ben Abd el-Jalil. Shakwa-l-gharib 'ani-l-awṭân ilâ 'ulamâ'-l-buldân de 'AYN AL-QUḌÂT AL-HAMADHĀNI (d. 525-1131), édité et traduite, avec introduction et notes. *Journal Asiatique*, 216, 1-76, 193-297, 1930. ISIS

XIIth Century (second half)

Konstantinowa, Alexandra. Ein englisches Bestiar des zwölften Jahrhunderts. Mit einem Vorwort von ADOLPH GOLDSCHMIDT. 32 p., 32 illus. *(Kunstwissenschaftliche Studien, 4)*. Berlin, Deutscher Kunstverlag, 1929. Rm. 4.50. ISIS

Description of a Leningrad bestiary which is probably of English origin and dates from the end of the twelfth century. The text is Latin, entitled

De omnium animantium descriptio sive historia naturalis. One hundred fourteen animals are described (reptiles, fishes, birds, mammals including whale and man). There is a miniature for each animal; the figures are beautiful but highly conventional. The other bestiaries of the twelfth to the fourteenth century describe much fewer animals, from 12 to 53; and, when illustrated, the number of illustrations is generally smaller than that of the animals. The bestiary which is nearest to the Leningrad one, is no. 107 of the J. PIERPONT MORGAN collection dating from c. 1170; see M. R. JAMES's description of it in the Morgan Catalogue of MSS. (vol. 1, 165-67, 2 ill., London, 1906). Miss KONSTANTINOWA concludes : « Die Hauptbedeutung des Leningrader Bestiars liegt wohl in dem überaus reichen ikonographischen Material, das manche Lücken in der Symbolik der mittelalterlichen Kunst ausfüllen kann. Es sind wohl genügend Physiologushandschriften und Bestiarien bekannt, aber die Vollständigkeit der Illustration lässt immer zu wünschen übrig. Die Leningrader Handschrift aber gibt die künstlerisch dargestellte Zoologie des Mittelalters in ihrer ganzen Fülle, und darin liegt auch ihr besonderer Wert. » G. S.

Bouyges, Maurice (S. J.) AVERROÈS. Tahafot at-tahafot. Texte arabe. XXXIX+679 p. (*Bibliotheca Arabica Scholasticorum*, série arabe, tome 3). Beyrouth, Imprimerie Catholique, 1930. ISIS

XIIIth Century (second half)

Bednarski, Adam. Die anatomischen Augenbilder in den Handschriften des ROGER BACON, JOHANN PECKHAM und WITELO. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 60-78, 11 fig., 1931. ISIS

Mayer, Claudius Franz. Die Personallehre in der Naturphilosophie von ALBERTUS MAGNUS. Ein Beitrag zur Geschichte des Konstitutionsbegriffs. *Kyklos*, 2, 191-257, 1929. ISIS

Pérez, José A. Sanchez. El libro de las cruces. Una obra astrológica que Don ALFONSO X, rey de España, mandó traducir del árabe. *Isis*, 14, 77-132, 4 fig., 1930. ISIS

Text of an astrological treatise translated from the Arabic into Spanish for ALFONSO EL SABIO by JUDAH BEN MOSES and JOHAN DASPA. It was written in 1259, being the earliest Spanish treatise on the subject (MS. no. 9294, Biblioteca Nacional, Madrid). The Arabic author was one 'UBAIDALLÂH as yet unidentified. The text was hitherto unknown because it had been catalogued under the name Oveidala, and not like the other Alfonsine translations under the king's name. G. S.

Wilms, P. Hieronymus. ALBERT DER GROSSE. 237 p., 1 pl. München, KÖSEL & PUSTET, 1930. ISIS

Reviewed by JOS. KOCH, *Deutsche Literaturzeitung*, 1, 2308-10, 1930.

XIVth Century (whole and first half)

Altrocchi, Rudolph. MICHELINO'S DANTE. *Speculum*, 15-59, 8 pl., 1931. ISIS

Apropos of the painting in Sante Maria del Fiore, representing DANTE, Florence, and the Divine Comedy, completed by DOMENICO DI MICHELINO in 1465. G. S.

Farinelli, Arturo. Der Aufstieg der Seele bei DANTE. Bibliothek Warburg, Vorträge, 1928-1929, 191-213. Leipzig, TEUBNER, 1930. ISIS

Lippmann, Edmund O. von. Ein neues alchemistisches Handbuch aus der Zeit um 1300. *Chemiker-Zeitung*, 54, 6 p., 1930. ISIS

Apropos of ZURETTI's edition of an anonymous Greek treatise on alchemy. Technical study of that text. The author supports ZURETTI's conclusion as to its date : beginning of the fourteenth century. G. S.

Norpoth, Leo. Zur Bio-, Bibliographie und Wissenschaftslehre des PIETRO D'ABANO, Mediziners, Philosophen und Astronomen in Padua. *Kyklos*, 3, 292-353, 1930. ISIS

Thorndike, Lynn. Advice from a physician to his sons. *Speculum*, 110-14, 1931. ISIS

Text edited from a British Museum MS. (Sloane, 3124) dated 1315 (or 1415). « Regimen conditum a Petro Fagarola in artibus et in medicina magistro quod ab ipso fuit missum a civitate Valencie ad civitatem Tholosanam duobus filiis ipsius studentibus in ipsa civitate Tholosana degentibus ». « It throws light upon such homely, daily matters as food, clothing and costume, both for summer and winter, day and night. It gives illuminating details as to personal hygiene, and suggests the error of the view that, because the mediaeval universities did not build stadiums, indulge in intercollegiate athletics, or institute departments of physical education, therefore the mediaeval student took little or no exercise. It illustrates both student and family life, our fourteenth century father being even more solicitous that his boys keep out of draughts and wear sufficiently warm clothing than a modern mother would be. It is naturally of much medical interest for its particular prescriptions of drugs and medicines as well as its counsels of hygiene and preventive medicine. By its insistence upon personal cleanliness and its advice to select lodgings removed from all foul smells of ditches or latrines, and insistence on the importance of breathing pure air, it confirms my contention made in a recent article that the Middle Ages have been unjustly represented as normally unsanitary. » G. S.

Thorndike, Lynn. A pest tractate before the black death. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 346-56, 1930. ISIS

« AUGUSTINE OF TRENT, in addition to being a member of the Order of Friars Eremites of St. Augustine, was a lecturer at the University of Perugia

and chaplain to NICHOLAS ABREIN from Brünn in Moravia, who was bishop of Trent from 1336 to 1347. To this prelate, on July 12, 1340, AUGUSTINE directed from Perugia an astrological and medical work discussing especially the sixth house of the figure for that year, 'which sixth house is called that of infirmities according to the astrologers'. AUGUSTINE's treatment is of especial interest because he deals with 'the pestilence of infirmities' which occurred in 1340. Both in his astrological explanation of these diseases and his instructions how to guard against them he seems a precursor of the pest tractates which were presently to be called forth in such numbers by the great mortality of 1348 and its many subsequent recurrences. His treatise deals with both past and future, interpreting astrologically what has already occurred in 1340, and making predictions for the remainder of the year. Yet his work is to be distinguished from those almanacs, ephemerides, tacuina, and other predictions for particular years based on the astrological doctrine of revolutions which became so common during the later middle ages. AUGUSTINE aims not merely to treat of the particular year, 1340, but to set forth general rules which will be useful to medical men and to mankind at large in other years. This was a prophetic instinct on his part, for before a decade had passed the great pestilence of 1348 was to call forth regimens of health similar to his in increasing numbers.»

Wiedemann, Eilhard. Beiträge zur Geschichte der Naturwissenschaften. LXXIX. IBN AL SCHÂṬIR, ein arabischer Astronom aus dem 14. Jahrhundert. *Sitzungsberichte der Physikalisch-medizinischen Sozietät zu Erlangen*, 60, 317-28, 1928. ISIS

XIVth Century (second half)

Gunther, Robert Theodore. CHAUCER and MESSAHALLA on the astrolabe. Now printed in full for the first time with the original illustrations. VIII+234 p., 73 fig. (*Early science in Oxford*, vol. 5). Oxford University Press, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 233-35, 1930.

XVth Century (whole and first half).

Müller, Gottfried. Aus mittelenglischen Medizintexten; Die Prosarezepte des Stockholmer Miszellankodex X, 90. *Kölner Anglistische Arbeiten* herausgegeben von HERBERT SCHÖFFLER, Band X. Leipzig, TAUCHNITZ, 1929. ISIS

Reviewed by LYNN THORNDIKE, *Isis*, 14, 436-37, 1930.

Thorndike, Lynn. GIOVANNI DE' FONTANA. *Isis*, 14, 221-22, 1930. ISIS

Apropos of FONTANA's treatise *De trigono balistario*, 1440.

Thorndike, Lynn. Science and thought in the fifteenth century.

Studies in the history of medicine and surgery, natural and mathematical science, philosophy and politics. XII+387 p., 10 pl. New York, Columbia University Press, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 235-40, 1930.

Wolfson, Harry Austryn. CRESCAS' Critique of ARISTOTLE. Problems of ARISTOTLE's physics in Jewish and Arabic philosophy. XVI+759 p. (Harvard Semitic series, vol. 6). Cambridge, Harvard University Press, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 240-44, 1930.

XVth Century (second half)

Stewart, Glenn. San Salvador Island to Cuba. A cruise in the track of COLUMBUS. *Geographical Review*, 124-30, 1 fig., 1931. ISIS

« It seems strange that in spite of the amount of study that has been given to CHRISTOPHER COLUMBUS in his discovery of the New World the actual points visited by him between his landfall and the Island of Cuba have never yet been positively determined. Most attempts at reconstructing this part of the first voyage have been made at the desk. I hoped to contribute to the solution by studying the problem in the field—by actually following the discoverer's course step by step in a small boat. » « The value of observations on the spot is illustrated by a point to which reference has already been made—the puzzle of the many islands. The observers who have studied the problem in the field naturally arrive at the correct explanation. The observations of RUDOLF CRONAU who went over the ground as far as the Ragged Islands in 1890 are in perfect accord with my own conclusions, though I was unaware of his work at the time of my voyage. »

(Library of Congress). Exhibit of books printed during the XVth century and known as incunabula. Selected from the Vollbehr collection purchased by Act of Congress 1930. List of Books. III+78 p. Washington, Government Printing Office, 1930. ISIS

Leonardo da Vinci.

Hildebrandt, Edmund. LEONARDO DA VINCI. Der Künstler und sein Werk. XV+351 p., 296 fig. Berlin, GROTE, 1927. ISIS

Reviewed by HERM. BEENKEN, *Deutsche Literaturzeitung*, 1, 2034-42, 1930.

McMurrich, J. Playfair. LEONARDO DA VINCI. The anatomist (1452-1519). Published for the Carnegie Institution of Washington. Baltimore, WILLIAMS and WILKINS, 1930. ISIS

Reviewed by F. M. G. DE FEYFER, *Bijdragen tot de Geschiedenis der Geneeskunde*, 10, 317-18, 1930; by HABERLING, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 54-55, 1931.

Schütz, Arthur. LEONARDO DA VINCI, der erste Riementechniker. *Beiträge zur Geschichte der Technik und Industrie*, 20, 147-48, 3 fig., 1930. ISIS

Stites, Raymond S. The bronzes of LEONARDO DA VINCI. *The Art Bulletin*, 12, 254-69, 28 fig., 1930. ISIS

XVIth Century (whole and first half)

C. — *Natural sciences*

Ley, Willy. KONRAD GESNER. Leben und Werk. VIII+154 p., port., 5 fig. *Münchener Beiträge zur Geschichte und Literatur der Naturwissenschaften und Medizin*, H. 15-16, 1929. ISIS

Nunn, George Emra. World map of FRANCESCO ROSELLI drawn on an oval projection and printed from a woodcut supplementing the fifteenth century maps in the second edition of the Isolario of BARTOLOMEO DALI SONETTI. Printed in Italy anno Domini MDXXXII. Described by GEORGE E. NUNN from a copy in the collection of GEORGE E. BEANS. III+30 p., front., illus., maps. Philadelphia, PALMER, 1928. ISIS

Weil, Ernst. REMACLUS FUSCH. Arzt, Botaniker und Pharmacologe (ca. 1510-1587). Bio-bibliographischer Versuch. XII p. *Münchener Beiträge zur Geschichte und Literatur der Naturwissenschaften und Medizin*, V. Sonderheft, 1928. ISIS

D. — *Medical sciences*

Achelis, J. D. Zur Grundstruktur der paracelsischen Naturwissenschaft. *Kyklos*, 1, 44-51, 1928. ISIS

Achelis, J. D. Zur Terminologie des Labyrinthus medicorum. *Acta paracelsica*, Heft 2, 33-39, München, 1930. ISIS

Aschner, Bernhard. Die Krebsbehandlung bei PARACELSUS. *Acta paracelsica*, Heft 2, 40-50 und 64, München, 1930. ISIS

Castiglioni, A. Il poema « Morbus Gallicus » di GIROLAMO FRACASTORO (1530-1930). 19 p., 4 pl. Torino, *Minerva Medica*, 2, 39, 1930. ISIS

Haberling, Wilhelm. Neues aus dem Leben des Danziger Arztes

und Dichters ALEXANDER VON SUCHTEN. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 117-23, 1931. ISIS

Koch, Richard. PARACELSUS in der Dauer der Zeit. *Acta paracelsica*, Heft, 2, 50-59, München, 1930. ISIS

Michelet, Léon. La vie d'AMBROISE PARÉ (chirurgien du Roy, écrivain). Preface by VICTOR PAUCHET. 160 p., 1 port., Paris, Librairie LE FRANÇOIS, 1930. ISIS

Reviewed by FRANCIS R. PACKARD, *Annals of Medical History*, 3, 125-26, 1931.

Stratman-Thomas, W. K. GIROLAMO FRACASTORO (1478-1563) and syphilis. *California and Western Medicine*, 33, 11 p., 2 fig., 1930. ISIS

Strunz, Franz. PARACELSUS zwischen den Zeiten. *Acta paracelsica*, Heft 2, 60-62, München, 1930. ISIS

Sudhoff, Karl. THEOPHRAST VON HOHENHEIM im Jahre 1526. *Acta paracelsica*, Heft 2, 62-63, München, 1930. ISIS

« Der Leser findet, vielleicht mit einigem Erstaunen, HANS HOLBEINS des Jüngeren berühmten ' Jüngeren Mann mit dem Schlapphut ' als Jugendbild des PARACELSUS wieder. Sein vermutliches Kopfschütteln wird nach folgenden Ausführungen zur Ruhe kommen können. »

Vesalius, Andreas. Epistola docens venam axillarem dextri cubiti in dolore laterali secandam. *Opuscula selecta neerlandicorum de arte medica*, vol. 8, p. XI-XXXII, 1-75, pl., Amsterdam, 1930. ISIS

This treatise on bloodletting in a case of pleuritis was published by VESALIUS at the age of 25, in Basel, 1539. It contains his first attack on the Galenic doctrines. VESALIUS proves that GALEN made inaccurate observations or extended unwarrantedly to man observations made on animals. The treatise was reprinted at Montferrat in 1544. It is now reprinted a second time together with a Dutch translation and an introduction (10 p.) by M. A. VAN ANDEL. G. S.

E. — *Alia*

Maynard, Theodore. De Soto and the conquistadores. XIII+297 p. New York, LONGMANS, 1931. ISIS

Reviewed by MARY WILHELMINE WILLIAMS, *American Historical Review*, 36, 446-47, 1931.

Racek, Jean. Contribution au problème de l'esthétique musicale chez DESCARTES. *Revue musicale*, 11e année, 289-301, 1930. ISIS

Reparaz, Gonçalo de (Júnior). Das Molucas à Flandres. (A expansão económica de Portugal no século XVI). *Seara Nova*, No. 137, p. 323-326, 1928. ISIS

Sinclair, Joseph H. The conquest of Peru as recorded by a member of the PIZARRO Expedition. Reproduced from the copy of the Seville edition of 1534 in the New York Public Library. With a translation and annotations by JOSEPH H. SINCLAIR. 47 p., 2 maps. New York. The New York Public Library, 1929. ISIS

Reviewed by A. POGO, *Isis*, 14, 244-50, 1930.

Weyde, A. J. van der. Bijdrage tot de kennis van den toestand van het hooger onderwijs in Belgie in het midden der XVI[e] eeuw. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 293-94, 1930. ISIS

XVIth Century (second half)

A. — *Mathematics*

Bombelli, Rafael. L'algebra. Opera di RAFAEL BOMBELLI da Bologna. Libri IV e V comprendenti « La Parte Geometrica » inedita tratta dal manoscritto B. 1569 della Biblioteca dell'Archiginnasio di Bologna. Pubblicata a cura di ETTORE BORTOLOTTI. N. 7 della collezione *Per la storia e la filosofia delle matematiche* diretta da FEDERIGO ENRIQUES. IV+302 p., facs., figs. Bologna, ZANICHELLI, 1929. ISIS

Reviewed by A. POGO, *Isis*, 14, 425-26, 1930.

B. — *Physical sciences and technology*

Brahe, Tycho. TYCHONIS BRAHE Dani: Opera omnia. Edidit I. L. E. DREYER. Tomus 15. V+54 p. Hauniae, Libraria Gyldendaliana, 1929. ISIS

Including index to the whole work. G. S.

Ferguson, John. (1837-1916). The secrets of ALEXIS. A sixteenth century collection of medical and technical receipts. *Proceedings of the Royal Society of Medicine*, 24, 1-22, 1930. ISIS

Very elaborate historical and bibliographical study of a book which enjoyed considerable popularity, witness some 69 editions of it, from the first which probably appeared in Venice about 1555 (lost) to the last in 1691. The author gives a long account of himself in the preface, according to which he must have been born about 1471, travelled considerably, and was well over 80 when he decided to publish his « secrets ». It has not been possible to identify him and we have no more means of checking his autobiography than that of his contemporary DENIS ZACHAIRE (for whom see TENNEY L.

DAVIS in *Isis*, 8, 287-99). The edition of this valuable paper was prepared with considerable care by Miss ELIZABETH H. ALEXANDER of Glasgow (*Isis*, 12, 147). G. S.

C. — *Natural sciences*

Hildebrandt, Hugo. Ein vergessener vogelkundlicher Schriftsteller des 16. Jahrhunderts. *J. Orn.* 78, 269-73, 1930. ISIS

Apropos of JOHANNES WIGAND (1523-1587).

D. — *Medical sciences*

Forestus, Petrus. De incerto fallaci urinarum iudicio. *Opuscula selecta Neerlandicorum de arte medica*, vol. 8, p. LIII-LXVII, 138-313, 2 pl., port. Amsterdam, 1930. ISIS

Reprint of the Latin text of the second book of FORREST's work first printed by PLANTIN in Leiden, 1589, together with the Dutch translation by C. NOLLENS, printed in Rotterdam, 1626, and with an introduction by M. A. VAN ANDEL. This treatise is largely devoted to criticize the exaggerations of uroscopists many of whom were nothing but quacks. One might compare it with the treatise published by CHRISTOPH CLAUSER in Zurich, 1531 (*Isis*, 7, 209, pl. 5). G. S.

Lommius, Jodocus. Observationum medicinalium, liber 1. *Opuscula selecta Neerlandicorum de arte medica*, vol. 8, p. XXXV-XLIX, 75-137, 1 pl., Amsterdam, 1930. ISIS

Reprint of the first book of the *Observationum medicinalium libri tres, quibus notae morborum omnium et quae de his possint haberi praesagia judiciaque proponuntur* (Antwerp, 1560), together with the English translation made by THOMAS DALE in 1732, and an introduction by F. M. G. DE FEIJFER. This treatise on fevers is interesting because it represents a return to the Hippocratic tradition as opposed to the Galenic which dominated sixteenth century thought. G. S.

Seckendorf, Ernst. Die erste gedruckte Nürnberger Medizinalordnung. Ein Beispiel für Entstehung und Inhalt früherer Medizinalordnungen. *Münch. med. Wschr.* 77, 1673-75, 1930. ISIS

Apropos of JOACHIM CAMERARIUS, 1571.

XVIIth Century (whole and first half)

A. — *Mathematics*

Agostini, Amedeo. Il problema inverso delle tangenti nelle opere di TORRICELLI. *Archeion*, 12, 33-37, 1930. ISIS

Agostini, Amedeo. La rettificazione di un qualunque arco di cerchio

mediante una cicloide fissa. *Periodico di matematiche*, 10, 36-38, 1 fig., 1930. ISIS

Henderson, J. The methods of construction of the earliest tables of logarithms. *Mathematical Gazette*, 15, 250-56, 1930. ISIS

Laumann, Julius. Der elementare Unterricht in Geometrie von 1660-1800. *Unterr.-Bl. f. Math. u. nat. Unt.*, 36, 317-20, 1930. ISIS

Laumann, Julius. Zur Geschichte des elementaren Unterrichts in Geometrie von 1600-1660 (1700). *Unterr. Bl. f. math. u. nat. Unt.*, 36, 294-97, 1930. ISIS

Sirven, J. Les années d'apprentissage de DESCARTES (1596-1628). 470 p. Paris, VRIN, 1928. ISIS

Reviewed in *Revue philosophique*, 151-52, 1930.

Webb, C. C. J. PASCAL's philosophy of religion. Oxford, Clarendon Press, 1929. ISIS

B. — *Physical sciences and technology*

Fahie, J. J. Memorials of GALILEO GALILEI, 1564-1642. Portraits and paintings, medals and medallions, busts and statues, monuments and mural inscriptions. With 20 portraits and 42 other illustrations. XXIV+172 p. Printed for the author: Leamington, Courier Press, September, 1929 (200 copies only printed, of which 105 are for sale). ISIS

Reviewed by G. SARTON, *Isis*, 14, 250-51, 1930.

Kepler, Johannes. JOHANNES KEPLER in seinen Briefen. Herausgegeben von MAX CASPAR und WALTHER VON DYCK. 2 Bände. Band 1. XXVIII+396 p. Band 2. XVI+348 p. München, OLDENBOURG, 1930. ISIS

Reviewed by R. A. S., *Nature*, 126, 835-36, 1930; by H. LUDENDORFF, *Deutsche Literaturzeitung*, 2, 231-33, 1931.

Lämmel, Rudolf. GALILEO GALILEI im Licht des zwanzigsten Jahrhunderts. 287 p., 16 pl. Berlin, FRANKE, 1927. ISIS

Plassmann, J. Was kann uns die Polyeder-Theorie lehren? Zum Gedächtnis von KEPLERS Tode (15. Nov. 1630). *D. Naturforsch.*, 7, 161-65, 1930. ISIS

Rossnagel, Paul. JOHANNES KEPLERS Weltbild und Erdenwandel. 80 p. (Reclams Universalbibliothek, Nr. 7082). Leipzig, 1930. ISIS

Schimank, Hans. KEPLERS « Weltengeheimeis » und « Neue Astronomie ». *Das Weltall*, 29, 161-66, 1930. ISIS

C. — *Natural sciences*

Aldenburgk, Johann Gregorius. Reise nach Brasilien 1623-26. 336 p. Haag, NIJHOFF, 1930. ISIS

Bayldon, Francis J. Voyage of LUIS VAEZ DE TORRES from the New Hebrides to the Moluccas. *J. and P. R. Australian Hist. S.*, 11, 158-194, 2 maps, 1925. ISIS

Bodilly, Ralph Burland. The voyage of Captain THOMAS JAMES for the discovery of the North-west Passage, 1631. 216 p. London, DENT, 1928. ISIS

Gudger, E. W. GEORGE MARCGRAVE, a postscript. 3 p. *Science*, 40, 507-09, 1914. ISIS

This article, antedating R. ZAUNICK's Nachtrag of 1916 (*Isis*, 14, 488), contained most of the items given by the latter. G. S.

Ordnance Survey. Map of XVIIth century England : with description, chronological tables, and a map of London, circa 1660. Scale : 16 miles to one inch. 24 p., map. Southampton, Ordnance Survey Office, 1930. ISIS

Stevens, Henry N. New light on the discovery of Australia, as revealed by the Journal of Captain DON DIEGO DE PRADO Y TOVAR. With annotated translations from the Spanish by GEORGE F. BARWICK. XII+261 p., maps. London, STEVENS, 1930. ISIS

Reviewed by ANNIE HELOISE ABEL-HENDERSON, *American Historical Review*, 36, 376-77, 1931; in *Geographical Review*, 176, 1931.

Ugolini, Ugolino. Un erbario bresciano del 1623. *Commentari dell'Ateneo di Brescia* per l'anno 1926, 216-236. Brescia, 1927. ISIS

D. — *Medical sciences*

Bergounioux, J. Deux professeurs en médecine de l'Université de

Cahors condamnés comme faux monnayeurs au XVII^e siècle. *Bulletin de la Société française d'histoire de la médecine*, 22, 315-74, 1928. ISIS

GUILLAUME GALTIER, docteur régent de 1623 à 1648, et BERNARD OUVRIER (ou DOUVRIER), professeur de 1648 à 1676. G. S.

Bilikiewicz, Tadeusz. JOHANN JONSTON (1603-1675) und seine Tätigkeit als Arzt. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 357-81, 1 fig., 1930. ISIS

(Cinchona). The tercentenary of cinchona in medicine. *Nature*, 126, 850-51, 975, 1930. ISIS

(Cinchona). Cinchona tercentenary celebration and exhibition at the Wellcome Medical Museum, 54, Wigmore-St. 114 p. London, The Wellcome Foundation, 1930. ISIS

Herringham, Sir Wilmot. The life and times of WILLIAM HARVEY. Oxford University Press, 1930. ISIS

Schadelbauer, Karl. Zu JOHANNES RÜMELIN und STEPHAN MICHELSPACHER. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 123-27, 1931. ISIS

Sigerist, Henry E. FABRIZIO BARTOLETTI. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 127-30, 1931. ISIS

« Der in Bologna, Pisa und Mantua tätige Arzt und Anatom BARTOLETTI hat den Vornamen FABRIZIO. Er ist 1586 geboren. Seine *Methodus in Dyspnœam* erschien 1633. »

E. — *Alia*

Adam, Charles. COMENIUS, 1593-1670. *Revue Bleue*, 68, 673-79, 709-715, 1930. ISIS

Andrada, Ruy Freyre de. Commentaries of RUY FREYRE DE ANDRADA, in which are related his exploits from the year 1619, in which he left this kingdom of Portugal as general of the sea of Ormuz, and coast of Persia, and Arabia, until his death. Edited with an introduction by C. R. BOXER. LVI+328 p. London, ROUTLEDGE, 1929. ISIS

Asín Palacios, Miguel. Comentarios de Don GARCÍA DE SILVA Y FIGUEROA de la embajada que de parte del rey de España don FELIPE III hizo al rey X ABAS DE PERSIA. 18 p. *Boletín d. Ac.*

de la Historia, vol. 92, 18 p. Madrid, Tipografia de Archivos, 1928. ISIS

Reviewed by LUCIEN BOUVAT, *Journal Asiatique*, 216, 366, 1930.

Bushnell, David I., jr. The five Monacan towns in Virginia, 1607. *Smithsonian Miscellaneous Collections*, 82, no. 12, 88 p., 14 pl. Washington, Smithsonian Institution, 1930. ISIS

Child, J. M. BARROW, NEWTON, and LEIBNIZ, in their relation to the discovery of the calculus. *Science Progress*, no. 98, 295-307, 1930. ISIS

Tend à prouver que BARROW est le vrai inventeur du calcul infinitésimal. J. P.

Lithgow, William (1582-1645). Rare adventures and painfull peregrinations. Edited by B. I. LAWRENCE. The Travellers' Library. London, CAPE, 1928. ISIS

Summers, Montague. The discovery of witches. A study of Master MATTHEW HOPKINS commonly call'd Witch Finder Generall. Together with the reprint of the Discovery of Witches from the rare original of 1647. 62 p. London, Cayme Press, 1928. ISIS

Webb, Clement C. J. PASCAL's philosophy of religion. Oxford University Press, 1930. ISIS

Young, Robert Fitzgibbon. COMENIUS and the Indians of New England. 27 p. London, School of Slavonic Studies, 40 Torrington Square, 1929. ISIS

XVIIth Century (second half)

A. — *Mathematics*

Dürr, Karl. Neue Beleuchtung einer Theorie von LEIBNIZ. Grundzüge des Logikkalküls. VIII+184 p. (*Leibniz-Archiv. Abh. d. Leibniz-Gesellsch.* In Verbindg. m. L. BIEBERBACH, K. BURDACH, A. v. HARNACK, M. LENZ, H. MAIER, F. MEINECKE, M. PLANCK, E. SPRANGER, C. STUMPF hrsg. v. PAUL RITTER. Bd. II.) Darmstadt, REICHL, 1930. ISIS

Reviewed by HEINRICH SCHOLZ, *Deutsche Literaturzeitung*, 1, 2119-21, 1930.

Lindemann, F. FABRI, BARROW und LEIBNITZ. *Stzgsber. Ak. München (math.-nat. Abt.)* 3, 273-284, 1927. ISIS

Pelseneer, Jean. Une opinion inédite de NEWTON sur « l'analyse des Anciens » à propos de l'Analysis geometrica de HUGO DE OMERIQUE (1698). *Isis*, 14, 155-65, 1930. ISIS

Prag, Adolf. JOHN WALLIS (1616-1703). Zur Ideengeschichte der Mathematik im 17. Jahrhundert. *Quellen und Studien zur Geschichte der Mathematik. Abteilung B : Studien*, 1, 381-412, 1930. ISIS

B. — *Physical sciences and technology*

Boyle, Robert. Der skeptische Chemiker. Verkürzt herausgegeben und übersetzt von EDUARD FÄRBER und MORITZ FÄRBER. 108 p. (*Ostwalds Klass d. ex. Wiss.* Nr. 229). Leipzig, Akademische Verlagsgesellschaft, 1929. ISIS

Chemnitius, F. ; Narr, P. Zur Geschichte des Poliergoldes. *Chem.-Ztg.*, 53, 573-74 ; 590-91, 1929. ISIS

Gunther, Robert Theodore. Early science in Oxford. Vol. VIII, The Cutler lectures of ROBERT HOOKE. VI+391 p., pl., figs. Oxford. Printed for the author, 1931. ISIS

Vols. VI and VII of this series, containing the Life and work of ROBERT HOOKE (1930), were reviewed in our Vol. 15, 174-77. Vol. VIII is a supplement to them. It includes a facsimile reprint of the six Cutler lectures delivered at Gresham College before the Royal Society, printed separately between 1674 and 1678 and provided with a general title page in 1679. The titles of these tracts are (abbreviated) :

1. An attempt to prove the motion of the earth from observations. 1674.

2. Animadversions on the first part of the *Machina coelestis* of JOHANNES HEVELIUS together with an explication of some instruments. 1674.

3. A description of helioscopes, and some other instruments. 1676.

4. Lampas : or, Descriptions of some mechanical improvements of lamps & waterpoises. Together with some other physical and mechanical discoveries. 1677.

5. Lectures and collections. Cometa. Microscopium. 1678.

6. Lectures *De potentia restitutiva*, or of Spring. Explaining the power of springing bodies. To which are added some collections. 1678.

The original pagination has unfortunately been destroyed and thus the original tables of contents have become less helpful than they would otherwise have been. It is true the editor has added a new index but that index is very meager.

It is pleasant to have these six rare tracts, but it is hardly excusable to reprint them in one way or another without explanation. To these 388 p. the editor has added a total of two short notes ! Producing books of this kind is a little too easy. Booksellers may possibly indulge in speculations of that kind but no scholar should be encouraged to publish a facsimile edition without providing it with all the necessary notes and with an intro-

duction explaining the meaning of the text, laying stress on its main points, and replacing it in its own background. G. S.

Morgan, J. R. ROBERT HOOKE (1635-1703). Part 1. *Science Progress*, no. 98, 282-94, 1930. ISIS

Nijland, A. A. CHRISTIAAN HUYGENS, in het bijzonder als astronom. *Christiaan Huygens ; internat. mathem. tijdschr.*, 7, 192-208, 1929. ISIS

Sampson, R. A., Conrady, A. E. On three HUYGENS lenses in the possession of the Royal Society of London. *Proc. Roy. Soc. Edinburgh*, 49, 289-99, 1928-29. ISIS

Sarton, George. Discovery of the dispersion of light and of the nature of color (1672). With facsimile reproduction (no. X) of NEWTON's New theory about light and colors (*Philosophical Transactions*, no. 80, February 19, 1672, p. 3075-87). *Isis*, 14, 326-41, 1930. ISIS

Speter, Max. Zur Geschichte des « Urin-Phosphors ». Das entdeckte Phosphor-Recept von BOYLE-HANCKEWITZ. *Chem.-Ztg.*, 53, 1005-06, 1929. ISIS

C. — *Natural sciences*

Schmid, Günther. Nelken und Tulpen in Halle im 17. Jahrhundert. *Flora*, S. 229-35, Dresden, 1929. ISIS

Apropos of the *Specimen florae Hallensis sive designatio plantarum hortui* M. J. G. O. (i.e., Magister JOHANN GOTTFRIED OLEARIUS) 1688.

Wilkinson, Clennell. WILLIAM DAMPIER. XII+257 p., ill., maps. London, LANE, 1929. ISIS

D. — *Medical sciences*

Boswell, Eleonore. The library of the Royal College of Physicians in the great fire (1666). *The Library*, 10, 313-26, 1930. ISIS

Collins, Samuel. SAMUEL COLLINS, Leibarzt des Zaren ALEKSEJ MICHAJLOVIC, Moskowitische Denkwürdigkeiten. In Übersetzung hrsg. v. WILHELM GRAF. XVI+102 p. (*Slawisch-Balt. Quellen* H. 4). Leipzig, HAESSEL, 1929. ISIS

Culpeper, Nicholas. Selections from the « English physician ». Com-

piled and illustrated with woodcuts by OLIVE WARD HUNT. 32 p. London, MORING, 1930. ISIS

Faucci, Ugo. Notizie biografiche del Dottor GIOVAN COSIMO BONOMO oriundo francese e cittadino di Livorno. 7 p. Livorno, 1930. ISIS

Kleij, J. J. van der. THOMAS SYDENHAM (1624-1689) en zijn verhandeling over de hysterie. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 271-78, 1930. ISIS

Weinberger, Bernhard Wolf. CHARLES ALLEN's « The operator for the teeth, » York, 1685 : the history of the first English dental publication. With corrections. *Journal of the American Dental Association*, 18, 67-76, 1931. ISIS

Young, James. MALPIGHI's *De pulmonibus*. *Proc. Roy. Soc. Med.* 23, (sect. history med., 1-11, 2 fig.), 1929. ISIS

E. — *Alia*

Crump, Phyllis Eirene. Nature in the age of LOUIS XIV. XV+224 p. London, ROUTLEDGE, 1928. ISIS

McKeon, Richard. The philosophy of SPINOZA : the unity of his thought. IX+345 p. New York, LONGMANS, GREEN, 1928. ISIS

Reviewed by G. DAWES HICKS, *Nature*, 125, 264-66, 1930.

Roth, Leon. SPINOZA. XVI+250 p. (Leaders of philosophy series). London, BENN, 1929. ISIS

Reviewed by G. DAWES HICKS, *Nature*, 125, 264-66, 1930.

Schurhammer, Georg (S. J.). Die Disputationen des P. COSME DE TORRES (S. J.) mit den Buddhisten in Yamaguchi im Jahre 1551. Nach den Briefen des P. TORRES und dem Protokoll seines Dolmetschers Br. JUAN FERNANDEZ (S. J.) XI+114 p. (*Mitteilungen der Deutsch. Ges. f. Natur- u. Völkerkunde Ostasiens*, Bd. 24, Teil A.) Leipzig, Asia Major, 1929. ISIS

Reviewed by ERICH SCHMITT, *OLZ*, 34, 72, 1931.

Spinoza. Œuvres traduites et annotées par CH. APPUHN. Tome III, Traité politique, lettres. 395 p. Paris, GARNIER, 1929. ISIS

XVIIIth Century (whole and first half)

B. — *Physical sciences and technology*

Diergart, Paul. Zwei Beiträge zur Erfindungsgeschichte des euro-

päischen Porzellans. *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik*, 13, 167-87, 1930. 1. Der Streit um TSCHIRNHAUS und BÖTTGER. 2. Einspruch gegen die geplante BÖTTGERbüste ohne TSCHIRNHAUS im Deutschen Museum zu München und dessen Begründung. ISIS

Hellman, C. Doris. GEORGE GRAHAM, maker of horological and astronomical instruments. *Popular Astronomy*, 39, 186-99, 1931. ISIS

Lippmann, Edmund O. von. Zur Frage nach der Herkunft der pflanzlichen Aschenbestandteile. *Chemiker-Zeitung*, 55, 2, 1931. ISIS

Apropos of MACQUER (1718-84).

Rohr, Moritz von. Weitere Aufschlüsse über das englische Brillenwesen im 18. Jahrhundert. *Z. ophtahlm. Opt.* 18, 43-48, 1930. ISIS

C. — *Natural sciences*

Carruthers, Douglas. The desert route to India. Being the journals of four travellers by the Great Desert Caravan Route between Aleppo and Basra, 1745-51. Edited by DOUGLAS CARRUTHERS. (Hakluyt Society, series II, vol. LXIII). XXXVI+196 p., map, 6 pl. London, 1929. ISIS

Reviewed by C. E. A. W. O., *Journal of the Royal Asiatic Society*, 203-04, 1931.

Franchini, Joseph. ANTONIO VALLISNERI (May 3, 1661-January 18, 1730) On the second centenary of his death. *Annals of Medical History*, 3, 58-68, 10 fig., pl., 1931. ISIS

Irsay, Stephen d'. ALBRECHT VON HALLER. Eine Studie zur Geistesgeschichte der Aufklärung. 98 p., 5 pl. (*Arbeiten des Instituts für Geschichte der Medizin an der Universität Leipzig*, Band 1). Leipzig, THIEME, 1930. ISIS

This is the firts attempt to give an account of HALLER's scientific activity and to place it in its environment. It is divided as follows : Vorbemerkung; Biographischer Rahmen; 1. Die gelehrte Aufklärung; 2. HALLER und VOLTAIRE — ein Weltanschauungskampf; 3. Anatomia animata; 4. Irritabilität; 5. Medizin; 6. Botanik und Pharmakognosie; 7. Sammelarbeit; 8. Auswirkungen; Bibliographische Bemerkungen.

Sarton, George. STEPHEN HALES's Library. (Query no. 15). *Isis*, 14, 422-23, 1930. ISIS

Schmid, Günther. LINNÉ-Dedichte. *Särtryck ur Svenska Linné-Sällskapets Årsskrift*, 12, 107-19, 1929. ISIS

Schmid, Günther; Freund, Hans. LINNÉ und Academia Naturae Curiosorum. *Särtryck ur Svenska Linné-Sällskapets Årsskrift*, 12, 124-52, 6 fig., 1930. ISIS

D. — *Medical sciences*

Caulfield, Ernest. The infant welfare movement in the eighteenth century. *Annals of Medical History*, 2, 660-96, 1930. ISIS

Daels, Frans. Jan Palfijn. *Vlaamsch geneesk. Tijdschr.*, Nr., 2-5, 27 p., 6 fig., 1927. ISIS

Irsay, Stephen d'. Scientific thought and enlightenment. *Kyklos*, 3, 136-46, 1930. ISIS

(Middelburg). Ordonnantie en reglement op de vroedvrouwen der stad Middelburg. *Bijdragen tot de geschiedenis der geneeskunde*, 11, 13-17, 1931. ISIS

(Palfijn, Jan). Palfijnhulde. Ghent, June 15, 1930. 58 p., 1 fig., 1930. ISIS

Seibels, Robert E. THOMAS DALE, M.D. (1700-50) of Charleston, S.C. *Annals of Medical History*, 3, 50-7, 7 fig., 1931. ISIS

XVIIIth Century (second half)

A. — *Mathematics*

Nielsen, Niels. Géomètres français sous la Révolution. VIII+250 p. Copenhagen, LEVIN & MUNKSGAARD, 1929. ($ 2.50). ISIS

Série de notes biographiques de ces géomètres : CARNOT, FOURIER, LAGRANGE, LAPLACE, LEGENDRE, MEUSNIER, MONGE, MONTUCLA et tant d'autres (environ 75 en tout) en ordre alphabétique. L'auteur a eu l'occasion de consulter presque tous leurs ouvrages mathématiques; à ce point de vue il a fait un travail de première main. Il est regrettable qu'il n'ait pas complété cet exposé analytique par une synthèse, car l'ordre alphabétique, si pratique qu'il soit, est d'autre part si arbitraire qu'il décourage le lecteur. On peut rapprocher de ce travail l'étude récemment publiée dans *Isis* par L. G. SIMONS :

The influence of French mathematicians at the end of the eighteenth century upon the teaching of mathematics in American colleges (vol. 15, 104-123, 1931). G. S.

C. — *Natural sciences*

Schuster, Julius. Ein LINNÉsches Dichtungsmotiv und GOETHES Metamorphosen-Elegie. *Forschungen u. Fortschritte*, 5, 99-100, 1929. ISIS

Walther, Johannes. GOETHE als Seher und Erforscher der Natur. Untersuchungen über GOETHES Stellung zu den Problemen der Natur. 332 p., 15 pl. Kaiserlich Leopoldinische Deutsche Akademie der Naturforscher zu Halle, 1931. ISIS

D. — *Medical sciences*

Abt, I. A. CHRISTOPHER WILHELM HUFELAND. *Annals of Medical History*, 3, 27-38, 1931. ISIS

Browne, George Buckston. Hunterian and other pictures in the Museum collection of the Royal College of Surgeons. 8 p. ISIS

Castiglioni, Arturo. Il viaggio d'un medico tedesco in Italia alla fine del settecento. *Rass. clin.-scient. Ist. Bio-chim. Ital.* 8, Nr. 8, 13 p., 1930. ISIS

Apropos of XAVER JANSEN.

Freeman, Walter. LEWIS MORGAN. The notebooks of a Tory medical student. *Annals of Medical History*, 2, 602-13, 2 fig., 1930. ISIS

Hérissay, Jacques. Une autopsie à Evreux sous le consulat (14 frimaire an IX-5 décembre 1800). *Bulletin de la société française d'histoire de la médecine*, 24, 263-72, 1930. ISIS

Hervé, Georges. JOHN HUNTER et ses travaux de génétique. *Bulletin de la société française d'histoire de la médecine*, 24, 229-38, 1930. ISIS

Linon, G. Note relative au traitement d'une fistule lacrymale. *Bulletin de la société française d'histoire de la médecine*, 24, 273-74, 1930. ISIS

Schmiz, Karl. Medizinstudium an der kurkölnischen Universität in Bonn (1777 bis 1797). *Proteus*, 1, 36-41, Bonn, 1931. ISIS

E. — *Alia*

Halévy, Elie. The growth of philosophic radicalism. Translated by

MARY MORRIS. XVIII+554 p. New York, MACMILLAN, 1928. ISIS

Reviewed by F. W. COKER, *American Historical Review*, 36, 385-86, 1931.

XIXth Century (whole and first half)

A. — *Mathematics*

Mack, Heinrich. Das Gaussmuseum in Braunschweig. *Museumskde.* N.F. 1, 122-25, 1930. ISIS

Pelseneer, J. La correspondance de N. BOWDITCH. Opinions de LE GENDRE. *Isis*, 14, 227-28, 1930. ISIS

Siber, Thaddäus (1774-1854). Main Lernen und Lehren. Autobiographische Aufzeichnungen. Hrsg. v. MAX ROTTMANNER. *Oberbayr. Arch. f. vaterl. Gesch.* 65, 83-225, 1927. ISIS

B. — *Physical sciences and technology*

Behaghel, Otto. Unbekannte Briefe LIEBIGS. *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik*, 13, 188-198, 1930. ISIS

Bloch, M. GERHARDT und LAURENT. BUGGE, *Das Buch der grossen Chemiker*, 2, 92-114, 2 pl., 1930. ISIS

CHARLES GERHARDT (1816-56), AUGUSTE LAURENT (1807-53).

Fischer, Hugo. Die Errichtung der Bleiwarenfabrik auf der Halsbrücker Hütte bei Freiberg i. Sa. *Beiträge zur Geschichte der Technik und Industrie*, 20, 126-29, 4 fig., 1930. ISIS

Garnett, Henry. Photographs of JOHN DALTON. *Nature*, 127, 201, 1931. ISIS

Gregory, Joshua C. The scientific achievements of Sir HUMPHRY DAVY. VIII+144 p. London, Oxford University Press, 1930. ISIS

Reviewed in *Nature*, 126, 840, 1930.

(Liebig). Das Liebigzimmer in Bad Salzhausen. *Z. angew. Chem.* 43, 613, 1930. ISIS

Martin, Thomas. FARADAY's Diary. *Nature*, 126, 812-14, 1930. ISIS

« It has now been announced that, to mark the forthcoming centenary of the discovery of electromagnetic induction, the Managers of the Royal Institution have resolved to publish a document of exceptional scientific interest and importance, FARADAY's ' Diary '. It may therefore be opportune to give some particulars of the manuscript and of how it came to be written. Scientific men have been aware of its existence for upwards of sixty years, and BENCE JONES, SILVANUS THOMPSON, and other writers on the life of FARADAY have consulted it for material and have quoted passages from it in their writings; but few of the present generation, to whom the name of FARADAY has become a household word, can be fully aware of the nature and extent, the scientific and biographical significance, and the extraordinary interest of these hitherto unpublished papers. »

Rohr, Moritz v. Ein neu aufgefundener Brief P. L. GUINANDS und Aufschlüsse daraus. *Forsch. z. Gesch. d. Opt.* 1, 222-41, 4 fig., 1930. ISIS

C. — *Natural sciences*

Andrée, Karl. KARL ERNST ADOLF VON HOFF als Schriftgelehrter und die Begründung der modernen Geologie. 28 p. (*Schriften d. Kgl. Deutschen Ges. zu Königsberg Pr.*, H. 4). Königsberg Pr. : GRÄFE u. UNZER, 1930. ISIS

Reviewed by ZAUNICK, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 29, 1931.

Brigham, Albert Perry. The centenary of the Royal Geographical Society. *Geographical Review*, 142-45, 1931. ISIS

Höfer, Horst. DANIEL ALBRECHT THAER (zur Wiederkehr seines 100. Todestages). *Mitt. Oekon. Ges. in Sachsen*, 47-61, 1928-29. ISIS

Lippmann, Edmund O. von. Vor hundert Jahren X. *Die Deutsche Zuckerindustrie*, Nr. 1, S. 18, 1931. ISIS

Apropos of MATHIEU DE DOMBASLE : Faits et observations sur la fabrication du sucre de betteraves (Paris, 1831).

Mill, Hugh Robert. The record of the Royal Geographical Society, 1830-1930. Published at the celebration of the society's centenary, October 1930. XVI+288 p., 35 pl. London, Royal Geographical Society, 1930. ISIS

Reviewed by R. N. R. B., *Nature*, 126, 679-80, 1930.

Schmid, Günther. GOETHE und VON SEELUS. *Ekkehard, Mitteilungsblatt deutscher Genealogischer Abende*, 6, 8 S., Halle a. S., 1930. ISIS

Apropos of the relations between GOETHE and the botanist VON SEELUS, otherwise unknown, at Weimar in 1831. G. S.

D. — *Medical sciences*

Bruck, Franz. SEMMELWEIS und VIRCHOW. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 387-88, 1930. ISIS

Dawson, Warren R. Memoir of THOMAS JOSEPH PETTIGREW, F.R.C.S., F.R.S., F.S.A. (1791-1865). *Medical Life*, 5-65, illus., 1931. ISIS

« Although my interest in THOMAS JOSEPH PETTIGREW first arose out of his Egyptological studies, these by no means constituted the limits of his activities. He was the friend and biographer of LETTSOM, surgeon to the Dukes of Kent and Sussex, as well as to the Duchess of Kent and her infant daughter, who afterwards became Queen VICTORIA, whom he vaccinated. He was librarian to the Duke of Sussex, first Professor of Anatomy at Charing Cross Hospital, surgeon to various other institutions, a prolific writer on medical biography, and the author of one of the best and fullest biographies of NELSON. In addition to all these activities, he was one of the founders of the British Archaeological Association, to whose publications he made many notable contributions. He numbered amongst his friends many of the most prominent artistic, literary and scientific men of his time. »

Dentz, F. Oudschans. Dr. CONSTANTIN HERING en CHRISTIAAN JOHANNES HERING. *De West-Indische Gids*, Juli-Augustus, 1930. ISIS

Reviewed by F. M. G. DE FEYFER, *Bijdragen tot de geschiedenis der geneeskunde*, 10, 296, 1930.

Hervé, Georges. Quelques documents sur CORVISART. *Bulletin de la société française d'histoire de la médecine*, 24, 239-53, 1930. ISIS

Hirschfeld, Ernst. Romantische Medizin. Zu einer künftigen Geschichte der naturphilosophischen Ära. *Kyklos*, 3, 1-89, 1930. ISIS

Hoeven, J. van der. Inleiding tot en demonstratie van eenige caricaturen en prenten betreffende GALL en diens leer. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 301-06, 2 fig., 1930. ISIS

Caricatures relative to FRANZ JOSEPH GALL and his doctrines.

Kushner, Martin D. CORRIGAN's aortic insufficiency. *Medical Life*, 676-688, (2-14), 3 fig., 1930. ISIS

Middleton, William Shainline. JOHN SYNG DORSEY (1783-1818). *Annals of Medical History*, 2, 387-601, 4 fig., 1930. ISIS

Pomerantz, Jacob. Melasma suprarenale. *Medical Life*, 689-700, (15-26), 3 pl., 1930. ISIS

Sticker, Georg. Eine Deutsche Akademie der Chirurgie im Jahre 1805. *Proteus*, 1, 41-45, Bonn, 1931. ISIS

Weiss, Arthur HODGKIN'S disease. *Medical Life*, 701-707 (27-33), 1930. ISIS

XIXth Century (second half)

A. — *Mathematics*

Dedekind, Richard. Gesammelte mathematische Werke. Hrsg. von ROBERT FRICKE, EMMY NOETHER, OYSTEIN ORE. I. Bd. 397 p. Braunschweig, VIEWEG, 1930. ISIS

Reviewed by L. BIEBERBACH, *Deutsche Literaturzeitung*, 1, 2302-2304, 1930.

Fraenkel, A. GEORG CANTOR. (3. III. 1845, St. Petersburg — 6. 1. 1918, Halle a.S.). *Jhrsber. Dtsch. Math.-Ver.*, 39, 189-266, 1930. ISIS

B. — *Physical sciences and technology*

Bernthsen, August. FRIEDRICH AUGUST KEKULÉ. *Z. angew. Chem.* 43, 719-22, 1930. ISIS

Collard, Auguste. ERNEST QUETELET (1825-1878). Un savant belge du XIX^e siècle. *Bulletin de la société belge d'astronomie, de météorologie et de physique du globe*, 302-316, 378-87, 1929. ISIS

Henrich, Ferdinand. ADOLF VON BAEYER (1835-1917). *Journ. Chem. Educ.*, 7, 1231-1248, 9 fig., 1930. ISIS

Hartley, Sir **Harold.** THEODORE WILLIAM RICHARDS. Memorial lecture. Delivered on April 25th, 1929. *Journal of the Chemical Society*, 1937-1969, 2 port., 1930. ISIS

Hassler, Fr. LUDWIG AUGUST RIEDINGER (1809-79). *Beiträge zur Geschichte der Technik und Industrie*, 20, 104-110, 7 fig., 1930. ISIS

Hendrichs, Franz. CARL FRIEDRICH ERN (1850-1924), der Bahnbrecher

für die Solinger Rasiermesserindustrie. *Beiträge zur Geschichte der Technik und Industrie*, 20, 47-62, 22 fig., 1930. ISIS

Josselin de Jong, R. de. Een Natuuronderzoeker, die tevens een sociaal hervormer was : ERNST ABBE (1840-1905). *Bijdragen tot de geschiedenis der geneeskunde*, 11, 1-8, 1 pl., 1931. ISIS

Schiaparelli, Giovanni Virginio. Le opere di G. V. SCHIAPARELLI. Pubblicate per cura della Reale Specola di Brera. Tomo 1. x+515 p., 19 pl. Tomo 2. IV+486 p., 30 pl. Milano, HOEPLI, 1930. ISIS

Reviewed by W. ALFRED PARR, *Nature*, 126, 675-77, 1930.

Stenger, Erich. Der Landschaftsphotograph und seine Arbeitsbehelfe zwischen 1860 und 1880. *Beiträge zur Geschichte der Technik und Industrie*, 20, 63-80, 43 fig., 1930. ISIS

Tanzer, Karl. JOSEF WERNDL (1831-89). Oesterreichs Waffenschmied. *Beiträge zur Geschichte der Technik und Industrie*, 20, 21-25, 6 fig., 1930. ISIS

Winderlich, Rudolf. LOTHAR MEYER und das periodische System. Ein Gedenkblatt zum 100. Geburtstag. *Aus der Heimat, Naturwiss. Mschr. d. Dt. Lehrerver. f. Naturk.* 43, 225-34, 1930. ISIS

C. — *Natural sciences*

Darwin, Charles. Down House. Here DARWIN thought and worked for forty years and died, 1882. 14 p. British Association for the Advancement of Science, (1929). ISIS

Down House and its grounds have been given to the B. A. A. S. by Mr. GEORGE BUCKSTON BROWNE together with an endowment for their maintenance and preservation for all time. Brief description of that House. G. S.

Osborn, Henry Fairfield. Fifty-two years of research, observation and publication, 1877-1929. A life adventure in breadth and depth. Edited by FLORENCE MILLIGAN. With complete bibliography, chronologic and classified by subject, 1877-1929. XII+160 p. New York, SCRIBNER'S, 1930. ISIS

This bibliography contains 801 titles which in the classified section are rearranged under these sixteen rubrics : geology, correlation and zoogeography, palaeontology, zoology, odontology, embryology, neurology, psychology, anthropology, evolution (biology, religion), eugenics (heredity), education, administration, conservation, biography, and miscellaneous.

There is also a summary of discoveries and new principles, a chapter on the fellowship of research with a list of academies, etc., in which membership is held, a list of medals and awards with the addresses made at the presentations, and a closing discussion of the impulse to research in geology and biology. The whole is a historical document of permanent value illustrative of an eminent career in a remarkable period of scientific advance in several fundamental scientific disciplines. C. A. K.

Pompeckj, Josef Felix. (1867-1930). Dem Andenken an JOSEF FELIX POMPECKJ. 29 p., port. Druck von J. J. AUGUSTIN in Glückstadt und Hamburg, 1930. ISIS

Includes biographical notices, a portrait, and lists of POMPECKJ's writings (1890 to 1929) and of his students. G. S.

Robb, Alfred A. Anticipation of WEGENER's hypothesis. *Nature*, 126, 841, 2 fig., 1930. ISIS

Curious anticipation by A. SNIDER : La Création et ses mystères dévoilés (1858). G. S.

Sarton, George. Discovery of the theory of natural selection. With facsimile reproductions (nos. VIII-IX) of DARWIN's and WALLACE's earliest publications on the subject (1859). *Isis*, 14, 133-54, 1930. ISIS

Willis, Bailey. THOMAS CHROWDER CHAMBERLIN (1843-1928). *Annual Report of the Smithsonian Institution*, 585-94, 1 pl., 1929. ISIS

Reprinted from *Bulletin of the Geological Society of America*, vol. 40, no. 1, March, 1929. Bibliography omitted.

D. — *Medical sciences*

Burr, Anna Robeson. WEIR MITCHELL : His life and letters. XII+ 424 p., 27 pl., frontispiece. New York City, DUFFIELD, 1929. ISIS

This is a beautifully printed and delightfully written account of SILAS WEIR MITCHELL (1829-1914), the distinguished physiologist, neurologist, and popular writer of Philadelphia. While there are copious passages referring to his scientific work, especially on snake venoms, neurology, and his famous « rest cure, » there is little critical appreciation of the significance of his scientific contributions. MITCHELL took great pride in his non-professional associations and moved in a memorable social group. Many letters are quoted to and from ROOSEVELT, TAFT, HOLMES, JAMES, EARL GREY, CARNEGIE, OSLER, NOGUCHI, WHITCOMB RILEY, and others. The volume includes much information on MITCHELL's father, Dr. JOHN KEARSLEY MITCHELL (1793-1858). There is also a bibliography of MITCHELL's writings, and a fair index. C. D. L.

Englert, Ludwig. JACOB HEINRICH HERMANN SCHWARTZ (1821-90).

Professor der Gynäkologie zu Göttingen. Eine biographische Skizze. *Kyklos*, 3, 147-82, 1930. ISIS

Hirschfeld, Ernst. VIRCHOW. *Kyklos*, 2, 106-16, 1929. ISIS

Scheunert, Gerhart. Kultur und Neurose am Ausgang des 19. Jahrhunderts. *Kyklos*, 3, 258-72, 1930. ISIS

Sigerist, Henry E. EMILE LITTRÉ über CHARLES DAREMBERG. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 382-84, 1930. ISIS

E. — *Alia*

Bode, Wilhelm von. Mein Leben. II. Bd. 264 p., 16 pl. Berlin, RECKENDORF, 1930. ISIS

Reviewed by K. K. EBERLEIN, *Deutsche Literaturzeitung*, 1, 2186-88, 1930.

Rayleigh, Lord. Lord BALFOUR in his relation to science. VIII+46 p. Cambridge, University Press, 1930. ISIS

Reviewed in *Nature*, 127, 87, 1931.

XXth Century

A. — *Mathematics*

Muir, Sir **Thomas.** Contributions to the history of determinants, 1900-1920. XXIV+408 p. London, BLACKIE and Son, 1930. ISIS

Reviewed by A. C. A., *Nature*, 126, 839, 1930. Contains subject index to the five volumes of MUIR's *History of Determinants*.

Reiser, Anton. ALBERT EINSTEIN, a biographical portrait, 225 p., portr. New York, ALBERT & CHARLES BONI, 1930. ISIS

B. — *Physical sciences and technology*

Carneal, Georgette. A conqueror of space : an authorized biography of the life and work of LEE DE FOREST. 296 p. New York, LIVERIGHT, 1931. ISIS

Martonne, Edmond de. La géodésie en Russie soviétique. *Revue scientifique*, 68e année, 737-45, 1930. ISIS

D. — *Medical sciences*

Flexner, Simon. HIDEYO NOGUCHI (1876-1928). *Annual Report of the Smithsonian Institution*, 595-608, 1 pl., 1929. ISIS

Reprinted from *Science*, June 28, 1929.

E. — *Alia*

Le Coq, Albert von. (1860-1930). Obituary with bibliography by OTTO STRÄUSS, *OLZ*, 33, 393-98, 1930. ISIS

PART II

HISTORICAL CLASSIFICATION

Including only the items which could not be included in Part I — the fundamental (centurial) classification. Hence a student of Muslim science, for example, should not peruse simply the notes collected below under the heading « Islam », he should examine as well those included above under the headings VIIth century, VIIIth century, etc.

CONTENTS :

I. — *Antiquity* : 1. Antiquity (generalities); 2. Egypt; 3. Babylonia and Assyria ; 4. Greece ; 5. Rome.
II. — *Middle Ages* : 6. Middle Ages (generalities) ; 7. Byzantium.
III. — *Oriental Science and Civilization* : 8. Asia (generalities ; Western Asia ; Central Asia ; Eastern Asia) ; 9. India ; 10. China ; 11. Japan ; 12. Israel ; 13. Iran ; 14. Islam.

I. ANTIQUITY

1. ANTIQUITY *(generalities)*

Evans, Sir Arthur. The palace of MINOS at Knossos. A comparative account of the successive stages of the early Cretan civilization as illustrated by the discoveries at Knossos. Vol. 3. The great transitional age in the northern and eastern sections of the palace : the most brilliant records of Minoan art and the evidences of an advanced religion. With 367 figures in the text, plans, 13 coloured and 11 supplementary plates . (Section of the « Grand staircase » and plans and perspective drawing of the « Domestic quarter » in pocket at the end of the volume). XXIV+525 p. London, MACMILLAN, 1930. ISIS

Rey, Abel. La science orientale avant les Grecs. La science dans l'antiquité. XVII+495 p. (*Bibliothèque de synthèse historique*. L'évolution de l'humanité. Synthèse collective. HENRI BERR, éd. (série complémentaire). Paris, La Renaissance du Livre, 1930. ISIS

Reviewed by O. NEUGEBAUER, *Deutsche Literaturzeitung*, 1, 2060-62, 1930.

Taylor, H. L. The origin and development of lenses in ancient times. *The Brit. Journ. of Physiol. Opt.* 4, 97-103, fig., 1929. ISIS

Virolleaud, Ch. La Syrie et la Phénicie dans la haute antiquité d'après les fouilles récentes. *Revue générale des sciences*, 41, 493-509, 13 fig., 1930. ISIS

2. — EGYPT

Budge, Sir E. A. Wallis. Amulets and superstitions. 544 p., 22 pl. London, University Press, 1930. ISIS

Capart, Jean. On falconry in ancient Egypt. *Isis*, 14, 222, 1930. ISIS

Capart, Jean. Propos sur l'art égyptien. XVI+307 p., 188 fig. Bruxelles, Edition de La Fondation Egyptologique Reine Elisabeth, 1931. ISIS

French translation of the author's American lectures of 1924-25. 1. Quelques chefs-d'œuvre de l'art égyptien; 2. Problèmes d'esthétique égyptienne; 3. Les merveilles de l'art industriel; 4. Les ruines de Thèbes; 5. Les belles histoires des fouilles; 6. La vallée des rois et la tombe de TOUTANKHAMON. These lectures form an excellent introduction to the study of Egyptian art. The book contains almost 200 figures, many of which are new or uncommon.
G. S.

Chace, Arnold Buffum; Bull, Ludlow; Manning, Henry Parker; Archibald, Raymond Clare. The Rhind mathematical papyrus. British Museum 10057 and 10058. 2 vols. Vol. 1, royal octavo, 28.5 × 20.5 cm.; 10+210 p.; Vol. 2, oblong folio, 28.5 × 36 cm., 288 p. Oberlin, Ohio, Mathematical Association of America, 1927-1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 251-55, 1930.

Erman, Adolf. Ägyptische Grammatik mit Schrifttafel, Paradigmen und Übungsstücken zum Selbststudium und zum Gebrauch in Vorlesungen. 4., völlig umgestaltete Aufl. XVI+309+10 p. (Porta Linguarum Orientalium). Ergänzungsband zum Erlernen der Schrift, Paradigmen und Übungsstücke, II+57+8 p., 1929. Schrifttafel. II+39 p., 1929. Berlin, REUTHER & REICHARD, 1928. ISIS

Reviewed by G. ROEDER, *OLZ*, 33, 870-71, 1930.

Filippoff, L. Les précurseurs d'HIPPARQUE. *Revue Scientifique*, 69, 38-47, 18 figs., 1931. ISIS

« La Revue Scientifique étant un organe de libre discussion scientifique, les opinions développées dans les divers articles et notes n'engagent que la responsabilité de leurs auteurs. » Néanmoins, M. JULES BAILLAUD, astronome titulaire à l'observatoire de Paris, est responsable, en sa qualité de rédacteur-en-chef de la *Revue Scientifique*, pour l'introduction de cet article dans la littérature scientifique. Le numéro du 24 janvier 1931, avec la reproduction, sur la couverture rose, d'un sphinx criocéphale pourra rivaliser avec le numéro du 1er septembre 1930 des Comptes Rendus, dont la couverture rose cache, sans rougir, la note de M. L. FILIPPOFF « Sur la détermination astronomique de l'époque de la disparition de l'Atlantide », présentée par M. GUILLAUME BIGOURDAN à l'Académie des Sciences (*Isis*, 16, 552).

Pour permettre au lecteur de se rendre compte du caractère des « Précurseurs d'HIPPARQUE », nous citerons la fin de l'article. « Les astronomes égyptiens connaissaient-ils donc les instruments optiques pour pénétrer les mystères du monde stellaire, comme l'affirmait positivement, déjà au XVIIIe siècle, DUTENS?... Mais la valeur exacte de la précession ne peut être obtenue, d'un autre côté, que si l'on connait *la cause* même du phénomène. Et nous savons, que NEWTON fut, officiellement, le premier, qui nous révéla — au XVIIIe siècle — que la précession est le résultat de l'attraction commune, exercée par le Soleil et la Lune sur l'ellipsoïde terrestre... Les astronomes égyptiens connaissaient-ils donc aussi la loi de l'attraction universelle quarante-huit siècles avant le génial auteur de « Principia », comme l'assurait déjà le même DUTENS? Car, comment auraient-ils pu trouver autrement les masses exactes du Soleil et de la Lune, nécessaires pour les calculs de la valeur de la précession et pour arriver à des résultats identiques à ceux qui furent obtenus tout dernièrement par nos meilleurs théoriciens? *Ignoramus*... Mais les faits et les chiffres sont là, nets, frappants, impitoyables pour notre orgueil, nous obligeant à rêver. Leur explication se trouve, pourtant, aisément dans l'origine et le caractère même de l'astronomie égyptienne. Et un jour, je tâcherai, peut-être ici-même, de donner la résolution du problème qui se pose à nous aujourd'hui. »

Il n'est pas difficile de prévoir cette explication du mystère : les habitants de l'Atlantide ont eu le soin, avant d'être noyés, c. à d. il y a 9187 ans (en 7256), de transmettre aux Égyptiens leur connaissances astronomiques. Parmi « les faits et les chiffres frappants, impitoyables pour notre orgueil », nous ne citerons que l'existence de « la lunette méridienne de cet ancien observatoire égyptien » (de Meroe) et la révélation que les astronomes d'Héliopolis « adoptaient pour la constante de la précession 50" 0880 tandis que celle de NEWCOMB adoptée universellement depuis l'an 1896 est 50" 2564. » Les étapes qui mènent à ces conclusions « frappantes et impitoyables » sont illustrées par des reproductions d'après FLAMMARION, *Les Étoiles et les Curiosités du Ciel*, et d'après MOREUX, *La Science mystérieuse des Pharaons;* par un dessin de l'auteur (fig. 16) portant la légende : « Les limites égyptiennes et les limites modernes de constellations Taureau-Bélier se confondent sur nos atlas célestes », faisant face à un autre dessin de l'auteur (fig. 17) servant à montrer que « la différence entre les limites anciennes et modernes des constellations Taureau-Bélier devient à peine sensible sur les cartes de Bonn »; et par la reproduction d'une photographie de la Carte du ciel (étoiles jusqu'à la 14e grandeur !!).

Parmi les nombreuses illustrations égyptologiques nous nous arrêterons à la fig. 4 — « La constellation du Taureau, parmi les principales constellations du ciel septentrional égyptien (d'après MASPERO) ». C'est le panneau nord du célèbre plafond de SETHI I; le taureau Meskheti représente la constellation de la Grande Ourse pour tout le monde sauf pour l'auteur. Pour lui, c'est la constellation zodiacale du Taureau connue « dès les époques préhistoriques »; ce Taureau « figure, en effet, parmi les principales constellation du ciel égyptien, que nous trouvons sur le plafond de Ramesséum (fig. 4) »; le lecteur notera que le mot « septentrional » a été supprimé; la substitution de « Ramesséum » pour « SETHI I » est due à MASPERO.

« Le Taureau Sacré, adoré sur la terre égyptienne depuis l'an 4100 était donc le symbole du signe du Zodiaque, qui marchait à cette époque en tête du troupeau céleste, car le Soleil rentrait alors dans le Taureau à l'équinoxe du printemps, en ouvrant ainsi l'année et faisant, par conséquent, du Taureau le premier signe du Zodiaque. Suivant l'usage ancien, en effet, l'année débutait au moment où le Soleil rencontrait le point vernal et l'on commençait l'énumération des signes du Zodiaque par celui dans lequel se faisait cette rencontre ». Malheureusement, les Égyptiens ne connaissaient pas, en 4100, nos signes du Zodiaque; la constellation circumpolaire de Meskheti n'était pas notre Taureau zodiacal; le bélier des décans 14 à 16 de la XVIII[e] et de la XIX[e] dynastie n'a pas encore été identifié avec notre Bélier zodiacal; l'année égyptienne ne débutait pas, « suivant l'usage ancien » (des habitants de l'Atlantide?) au moment où le Soleil rencontrait le point vernal inconnu aux Égyptiens du cinquième millénaire avant J.-C.; l'année égyptienne commençait avec le lever héliaque de Sothis-Sirius — et ce lever coincidait, vers 3200, à l'insu des Égyptiens, avec le solstice d'été dont ils s'occupaient aussi peu que de l'équinoxe de printemps.

Un des dogmes indiscutables et inébranlables de l'auteur est le suivant : les Égyptiens connaissaient exactement les courbes qui représentent les limites de nos constellations dans l'atlas d'ARGELANDER, *Uranometria Nova*. Il n'est pas surprenant que les calculs de l'auteur, basés sur une série d'hypothèses naïves, lui ont permis d'arriver à ses conclusions « frappantes, impitoyables pour notre orgueil, nous obligeant à rêver ».

La *Revue Scientifique* est sans doute responsable pour le côté purement typographique de l'article; « Antilka Itimmelscilder » et « Handwöterbucg d. Eyblis » présentent autant d'erreurs d'impression par mot que les citations mutilées empruntées à la langue de PLATON. A. P.

Humbert, Paul. Recherches sur les sources égyptiennes de la littérature sapientiale d'Israël. 195 p. *Mémoires de l'Université de Neuchâtel*, 7, Neuchâtel, 1929. ISIS

Reviewed by ADOLPHE LODS, *Revue de l'histoire des religions*, 101, 69-76, 1930; by CURT KUHL, *OLZ*, 33, 185-88, 1930.

Kees, Hermann. Die Himmelsreise im ägyptischen Totenglauben. *Bibliothek Warburg Vorträge*, 1928-1929, 1-20, 1930. ISIS

Lint, Jan Gerard de. Der Papyrus Edwin Smith. Ein Beitrag zur

ägyptischen Medizin im 3. vorchr. Jahrtausend. *Proteus*, 1, 19-25, Bonn, 1931. ISIS

Apropos of BREASTED's edition fully reviewed in *Isis*, 15, 355-67, 1931.

Montet, Pierre. Byblos et l'Egypte. Quatre campagnes et fouilles à Gebeil 1921, 1922, 1923, 1924, VI+317 p., atlas : 167 pl. (Haut-Commissariat de la République française en Syrie et au Liban. Service des Antiquités et des Beaux-Arts. Bibliothèque archéologique et historique, XI). Paris, GEUTHNER, 1928-29. ISIS

Reviewed by LUDWIG BORCHARDT, *OLZ*, 34, 24-35, 1931.

Murray, M. A. The bundle of life. Ancient Egypt, part 3, 65-73, 22 figs., 1930. ISIS

« I have tried to show that the Bundle of Life in which 'the Lord thy God' was bound belonged to the King and to the people in general, but it must also be remembered that each individual could possess an External Soul which, like that of the King, was housed in the placenta or umbilical cord; this, as is still done in parts of Africa, was preserved and wrapped up in cloth. Here, then, is the explanation of the little bundle-amulets, wrapped in cloth or papyrus, which are found in the New Kingdom, and which are represented in the Old Kingdom. »... « The umbilical cord, if it contained the soul of the owner, would be excessively precious to him and would hardly be worn at all times for fear of loss and consequent disaster, it would be kept in a safe place and only brought out on great occasions. But an object in which the bandaging had become stylised and was merely an imitation of the actual object could be worn at any time. The Bundle of Life was therefore not a « façon de parler » on the part of Abigail but a living belief, and one which must have had great influence on her own thoughts and those of the man to whom she spoke. »

Neugebauer, O. Arithmetik und Rechentechnik der Ägypter. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 301-80, 1930. ISIS

Niemann, W. B. Von Altägyptischer Technik. *Beiträge zur Geschichte der Technik und Industrie*, 20, 99-103, 8 fig., 1930. ISIS

Pendlebury, J. D. S. Aegyptiaca. A catalogue of Egyptian objects in the Aegean area. With a foreword by H. R. HALL. XIX+121 p., 4 pl., 1 map. Cambridge, Univ. Press, 1930. ISIS

Reviewed by MAX PIEPER, *DLZ*, 1, 2414-18, 1930; by VAL. MÜLLER, *OLZ*, 33, 991-92, 1930.

Pogo, A. The astronomical ceiling decoration in the tomb of SENMUT (XVIIIth dynasty). *Isis*, 14, 301-25, 10 pl., 4 fig., 1930. ISIS

« The recently discovered astronomical ceiling decoration in the tomb

of SENMUT (XVIIIth dynasty; about 1500 B.C.) follows, in general, the same prototype as the well-known ceiling in the Ramesseum (XIXth dynasty; about 1250 B.C.); the ceiling of SETI I (XIXth dynasty; about 1300 B.C.) follows a different tradition. Both traditions contain elements of different antiquity. Important new features of the SENMUT ceiling are the 12 monthly circles subdivided into 24 hourly sectors (unfinished « monthly star-charts »?), and the two « meridian cords » (connected with the ceremony of the « stretching of the cord » involving the observation of an upper culmination of Zeta Ursae Majoris?). For a tentative identification of several stars and constellations represented in the circumpolar group of the northern panel or mentioned in the decanologue of the southern panel, a celestial globe was used. The precession of the equinoxes was taken care of by the drilling of several pairs of polar holes; the latitude of Thebes was taken as the altitude of the pole. A repetition of the experiment in a ZEISS planetarium is suggested. The possibility of an exact dating of the three ceilings of the XVIIIth and XIXth dynasties by the use of their planetary data is pointed out. »

Sarton, George. IMHOTEP. Two statuettes in the Boston Museum. *Isis*, 14, 226, pl., 1930. ISIS

Schäfer, Heinrich. Von ägyptischer Kunst. Eine Grundlage. 3., neugestaltete und stark vermehrte Aufl. (6. Tausend). XVI+424 p., 268 fig., 62 pl. Leipzig, HINRICHS, 1930. ISIS

Reviewed by H. SPONSHEIMER in *OLZ*, 34, 109-14, 1931.

Schäfer, Heinrich. Weiteres zum Bogenschiessen im alten Ägypten. *OLZ*, 34, 89-96, 1931. ISIS

Sethe, Kurt. Altägyptische Vorstellungen vom Lauf der Sonne. 28 p. (*Sitzungsber. der Preuss. Akademie der Wissenschaften, phil.-histor. Klasse* 1928, XXII). Berlin, DE GRUYTER, 1928. ISIS

Reviewed by G. VAN DER LEEUW, *OLZ*, 53, 435-36, 1930.

Vogel, Kurt. Die Grundlagen der ägyptischen Arithmetik in ihrem Zusammenhang mit der 2:n—Tabelle des Papyrus Rhind. VI+211 p. Munich, BECKSTEIN, MÜLLER, 1929. ISIS

Reviewed by R. C. ARCHIBALD, *Isis*, 14, 430-33, 1930; by R. B. MCCLENON, *Bull. Am. Math. Soc.*, 37, 144-46, 1931.

Vogel, Kurt. The truncated pyramid in Egyptian mathematics. *Journal of Egyptian Archaeology*, 16, 242-49, 1930. ISIS

« We have two alternatives. If we see in the formula of 1850 B.C. the correctly derived formula for the truncated pyramid then we must allow to the Egyptians, side by side with a highly developed art of experiment, the arithmetical-algebraical equipment necessary to such a derivation in

its full extent. On the other hand if my suggested explanation be accepted we are no longer compelled to attribute to the Egyptians constructions and mental processes outside their ken. This remarkable problem will then be fully in place in a treatise of this date as an example of how men, while consciously striving after accurate knowledge though still working on the lines of the research indicated by ARCHIMEDES, succeeded in dealing with the problems of everyday life. »

3. — BABYLONIA AND ASSYRIA

Archibald, R. C. Babylonian mathematics. *Science*, 73, 68, 1931. ISIS

Apropos of G. A. MILLER's statements in *Science*, 72, 601, 1930, concerning the sexagesimal division of a circle. A. P.

Frank, Carl. Strassburger Keilschrifttexte in sumerischer und babylonischer Sprache. 36+XX p. (*Schriften der Strassburger Wissenschaftlichen Gesellschaft in Heidelberg*. Neue Floge, 9. Heft.) Berlin, W. DE GRUYTER, 1928. ISIS

Reviewed by A. DEIMEL, *OLZ*, 33, 30-32, 1930. With additional notes on the mathematical texts by O. NEUGEBAUER, (*OLZ*, 33, 32-33, 1930), and by C. FRANK (*OLZ*, 33, 587, 1930). G. S.

Gressmann, Hugo (1877-1927). The tower of Babel. Edited with a preface by JULIAN OBERMANN. XXIII+92 p., 11 pl. (The Hilda Stich Strook Lectures). New York, Jewish Institute of Religion, 1928. ISIS

Reviewed by W. ANDRAE, *OLZ*, 33, 751-53, 1930.

Koschaker, Paul. Neue keilschriftliche Rechtsurkunden aus der el-Amarna-Zeit. X+184 p. (*Abhandl. d. philolog.-hist. Klasse der sächs. Akad. d. Wissensch.*, 39, 5). Leipzig, HIRZEL, 1928. ISIS

Reviewed by M. SCHORR, *OLZ*, 33, 514-20, 1930.

Langdon, Stephen. Ausgrabungen in Babylonien seit 1918. Nach dem Manuskript des Verf. übers. von F. H. WEISSBACH. 77 p., 27 fig. (*Der Alte Orient*, 26). Leipzig, HINRICHS, 1927. ISIS

Reviewed by JULIUS JORDAN, *OLZ*, 34, 35-36, 1931.

Legrain, Léon. The culture of the Babylonians from their seals in the collection of the Museum. Vol. 1 : text, 365 p. ; vol. 2 : 64 pl. (*Publication of the Babylonian section*, vol. 14). Philadelphia, University Museum, 1925. ISIS

Reviewed by V. CHRISTIAN, *OLZ*, 33, 512-14, 1930.

Miller, G. A. Babylonian mathematics. *Science*, 72, 601-02, 1930. ISIS

« The Babylonian mathematics is of special interest in view of the fact that our division of the circle into 360 parts called degrees, and our division of the degree and the hour into 60 parts called minutes and of the minute into 60 parts called seconds can be traced back thereto. It has often been stated that the Babylonians employed a sexagesimal system of numerical notation. » « Since the Babylonians commonly employed only two distinct symbols to represent the first 59 natural numbers, viz., a symbol for unity and a symbol for ten, their system of numerical notation can be more properly called a decimal system than a sexagesimal system. » For R. C. ARCHIBALD's criticism of the statements concerning the division of a circle, see *Science*, 73, 68, 1931. A. P.

Neugebauer, O. Beiträge zur Geschichte der babylonischen Arithmetik. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 120-30, 3 fig., 1930. ISIS

1. Verteilung in arithmetischer Progression; 2. Gleichungen mit zwei Unbekannten; 3. Quadratische Gleichungen; 4. Schlussbemerkung.

Neugebauer, O. Sexagesimalsystem und babylonische Bruchrechnung. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 183-93, 1930. ISIS

Schaumberger, Johann B. Die Chronologie der HAMMURABI-Zeit nach neueren Forschungen. *Biblica*, 10, 332-62, 1929. ISIS

Reviewed by ED. MAHLER, *OLZ*, 33, 622-24, 1930.

Schuster, H. S. Quadratische Gleichungen der Seleukidenzeit aus Uruk. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 194-200, 1930. ISIS

Thompson, R. Campbell. Assyrian prescriptions for diseases of the ears. *Journal of the Royal Asiatic Society*, 1-25, 1931. ISIS

4. — GREECE

Baumann, E. D. An antique physiological problem. *Annals of Medical History*, 3, 21-26, 1931. ISIS

Apropos of the question « whether ingested liquids passed to the stomach or to the lungs. »

Falco, Vittorio de. Beiträge zur kritischen Textgestaltung des AUTOLYKOS und des HYPSIKLES. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 278-300, 1930. ISIS

Glotz, Gustave. La cité grecque. XXII+476 p. (L'évolution de l'humanité). Paris, La Renaissance du Livre, 1928. ISIS

Reviewed by FRITZ TAEGER, *OLZ*, 33, 418, 1930.

Jablonski, Walter. Die Theorie des Sehens im griechischen Altertume bis auf ARISTOTELES (Zur Geschichte der Theorie des Sehens I). *Sudhoffs Archiv für Geschichte der Medizin*, 23, 306-31, 1930. ISIS

Kahrstedt, Ulrich. Syrische Territorien in hellenistischer Zeit. 156 p., 6 maps. (*Abhandlungen der Ges. d. Wiss. zu Göttingen, philol.-hist. Klasse*, Bd. 19, 2.) Berlin, WEIDMANN, 1926. ISIS

Reviewed by O. LEUZE, *OLZ*, 33, 742-47, 1930.

Liddell, Henry George ; Scott, Robert. A Greek-English lexicon. A new edition revised and augmented throughout by HENRY STUART JONES. With the assistance of RODERICK MCKENZIE. And with the co-operation of many scholars. Part 5 : *θησαυροποιέω — κώψ*, p. 801-1020. Oxford, Clarendon Press, 1930. (Oxford University Press, 114 Fifth Ave., New York, $ 3.50). ISIS

It is pleasant to be able to report the steady progress of this splendid undertaking which was fully reviewed when the first part appeared in 1925 (*Isis*, 8, 200-2). With this part one half of the dictionary is completed. If the work is continued on the same scale and with the same speed, it will include some 2040 p. and be completed in 1935. The type is very small, yet very clear; there are more than 2500 words to a page : the number of examples quoted is truly immense. As half of the work is now available it is beginning to be very serviceable. G. S.

Regenbogen, Otto. Eine Forschungsmethode antiker Naturwissenschaft. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 131-82, 1930. ISIS

Vogel, Kurt. Die algebraischen Probleme des P. Mich. 620. *Classical Philology*, 25, 373-75, 1930. ISIS

« Der von ROBBINS veröffentlichte P. Mich. 620 ist für die Geschichte der Mathematik ungemein wertvoll, da er über algebraische Aufgaben und Methoden aus der Zeit vor DIOPHANTOS berichtet, mit dem bisher unsere Kenntnis griechischer Algebra erst beginnt. Die 3 nur unvollständig erhaltenen Aufgaben wurden von ROBBINS, was Problemstellung und Lösung anlangt, ohne Zweifel richtig wiederhergestellt und interpretiert. Nur für zwei noch unbefriedigende Punkte möchte ich hier eine Aenderung vorschlagen. » « Es wäre der Mühe wert, die Beziehungen aufzusuchen zwischen den vorliegenden Aufgaben und der Algebra des DIOPHANTOS sowie der ägyptischen Mathematik des mittleren Reiches, gegenüber der neben gemeinsamen Zügen auch eine deutliche Fortentwicklung zu beobachten ist. » To

the reprints distributed by the author are added seven lines of notes in MS.
G. S.

5. — ROME

Altheim, Franz. Griechische Götter im alten Rom. 216 p. (*Religionsgeschichtl. Versuche u. Vorarbeiten*. Begr. v. ALBR. DIETERICH u. RICH. WÜNSCH. In Verbindg. m. L. DEUBNER hrsg. v. L. MALTEN u. O. WEINREICH. Bd. XXII, H. 1). Giessen, TÖPELMANN, 1930. ISIS

Reviewed by MARTIN P. NILSSON, *Deutsche Literaturzeitung*, 1, 2224-27, 1930.

Collingwood, R. G. The archaeology of Roman Britain. XVI+293 p., 8 pl. (*Methuen's Handbooks of Archaeology.*) London, 1930. ISIS

Reviewed in *Nature*, 127, 9, 1931.

Sudhoff, Karl. Ein bisher unbeachteter ärztlicher Fund aus Römerzeit im deutschen Rheinland. *Proteus*, 1, 25-30, Bonn, 1931. ISIS

Van Sickle, C. E. The repair of roads in Spain under the Roman Empire. *Classical Philology*, 24, 77-88, 1929. ISIS

« The points to be considered are: (1) how often any given piece of road was repaired; (2) what the nature of the repairs was in each case; (3) what emperors were most active in road-building; and (4) what historical significance may be attached to the data which have been brought to light. »

« In general, it may be said that the curve of efficiency in Spanish road-repairs agrees remarkably well with what we know about that of the imperial government generally. The good government of AUGUSTUS and TIBERIUS, the profligacy of NERO, the energetic administration of the FLAVIANS, and of NERVA, TRAJAN, and HADRIAN; the *laissez faire* policy of ANTONINUS PIUS and the calamitous reign of M. AURELIUS, with the debauched and inefficient rule of his worthless son—all leave unmistakable traces upon the administration of the Spanish roads. Nor is the case different in the third and fourth centuries. Every recovery of prosperity and energy by the government has left its traces in an increased number of milestone inscriptions; and the failure of further examples in the later fourth century is eloquent testimony of the impending overthrow of the Empire. »

II. MIDDLE AGES

6. — MIDDLE AGES *(generalities)*

Baltrusaitis, Jurgis. Etudes sur l'art médiéval en Géorgie et en Arménie. Preface by HENRI FOCILLON. 109 p., 120 figs., 101 pl. Paris, LEROUX, 1929. ISIS

Reviewed by O. W. *Journal of the Royal Asiatic Society*, 231-32, 1931; by R. PFISTER, *Revue des arts asiatiques*, 6, 124-25, 1930; by K. MLAKER, *OLZ*, 34, 161-33, 1931.

Bock, August Wilhelm. Diätetische Wundbehandlung im Mittelalter. *Kyklos*, 2, 258-71, 1929. ISIS

« Est autem triplex instrumentum medicine per quod medicus diligenter medetur : scilicet dieta, potio et cyrurgicum instrumentum. Primo enim eger est dietandus, secundo potionandus; tertio si quid superfluum est in ipso quod per predictum instrumentum, scilicet potionem dietam nequeat removeri, per cyrurgicum instrumentum est exstirpandum. Dieser Satz aus der Einleitung der Viermeisterglosse, der in veränderter Gestalt mehrfach in den mittelalterlichen chirurgischen Schriften bis zu MONDEVILLE auftaucht, bezeichnet eindringlich genug die Bedeutung, die man ursprünglich der diätetischen Behandlung der chirurgischen Krankheiten beimass. »

Bréhier, Louis. L'art en France des invasions barbares à l'époque romane. 211 p., 16 pl. Paris, La Renaissance du Livre, 1930. ISIS

Reviewed by PAUL DESCHAMPS, *Journal des Savants*, 396-409, 1930.

Brogger, Anton Wilhelm. Ancient emigrants. A history of the Norse settlements of Scotland. XII+208 p. Oxford, Clarendon Press, 1929. ISIS

Budge, Sir E. A. Wallis. The book of the saints of the Ethiopian Church. A translation of the Ethiopic Synaxarium made from the manuscripts Oriental 660 and 661 in the British Museum. 4 volumes. Vol. 1, LXXXVII+p. 1-308 ; vol. 2, XVIII+p. 309-660 ; vol. 3, XVI+p. 661-953 ; vol. 4, XVIII+p. 954-1287 ; index p. 1289-1333. Cambridge, University Press, 1928. ISIS

Reviewed by MARCEL COHEN, *Revue de l'histoire des religions*, 99, 307-10, 1929.

Cornell, Henrik. Biblia pauperum. XVI+372 p., 8+72 pl., 37 figs. Stockholm, THULE-TRYCK, 1925. ISIS

Reviewed by MARCEL AUBERT, *Bibliothèque de l'école des chartes*, 88, 332-34, 1927.

Coulton, George Gordon. Life in the Middle Ages. Selected, translated and annotated by G. G. COULTON. Vol. 4. Monks, friars, and nuns. XV+395 p. Cambridge University Press, 1928. ISIS

Davies, John David Griffith ; Worts, F. R. England in the Middle Ages. XIII+298 p. London, KNOPF, 1928. ISIS

Deschamps, Paul. Étude sur la paléographie des inscriptions lapidaires de la fin de l'époque Mérovingienne aux dernières années du XII^e siècle. 86 p., 35 pl., 61 figs. Paris, Société générale d'Imprimerie et d'Édition, 1929. ISIS

Reviewed by MEYER SCHAPIRO, *The Art Bulletin*, 12, 101-09, 1930; by LOUIS BRÉHIER, *Journal des Savants*, 280-81, 1930.

Dunbar, H. Flanders. Symbolism in mediaeval thought and its consummation in the Divine Comedy. XVII+563 p. New Haven, Yale University Press, 1929. ISIS

Reviewed by C. H. GRANDGENT, *Speculum*, 107-11, 1930.

Enlart, Camille (1862-1927). Les monuments des croisés dans le royaume de Jérusalem. Architecture religieuse et civile. 2 vols. XV+216 p., 16 fig.; 541 p., 70 fig., maps, 196 pl. (Haut-Commissariat de la République française en Syrie et au Liban. *Bibliothèque archéologique et historique*, tomes VII et VIII.) Paris, GEUTHNER 1925-28. ISIS

Reviewed by VINCENT FLIPO, *Journal des Savants*, 81-90, 2 ills., 1930.

Erben, Wilhelm. Kriegsgeschichte des Mittelalters. VIII+136 p. (*Histor. Zeitschr.* Beiheft 16.) München, OLDENBOURG, 1929. ISIS

Reviewed by KARL HADANK, *Deutsche Literaturzeitung*, 1, 1513-24, 1930.

Fliche, Augustin. La Chrétienté Médiévale (395-1254). XVIII+501 p. (Tome VII de l'Histoire du Monde publiée sous la direction de E. CAVAIGNAC). Paris, DE BOCCARD, 1929. ISIS

Reviewed by P. H., *Revue de l'histoire des religions*, 99, 326-27, 1929; by P. E. SCHRAMM, *DLZ*, 1, 227-29, 1930.

Foligno, Cesare. Latin thought during the Middle Ages. VIII+120 p. Oxford, Clarendon Press, 1929. ISIS

Reviewed by BROTHER GILES, *Speculum*, 443-45, 1930; by JAMES F. WILLARD, *American Historical Review*, 35, 391, 1930.

Gal, Ladislas. L'architecture religieuse en Hongrie, du XI^e au XIII^e siècles. XV+300 p., ills., maps. (*Etudes d'art et d'archéologie* publiées sous la direction d'HENRI FOCILLON). Paris, LEROUX, 1929. ISIS

Reviewed by F. MACLER, *Revue de l'Histoire des Religions*, 100, 250-52, 1929.

Grabmann, Martin. Die Proklosübersetzungen des WILHELM VON

MOERBEKE und ihre Verwertung in der lateinischen Literatur des Mittelalters. HEISENBERG-Festschrift — *Byzantinische Zeitschrift*, 30, 78-88, 1930. ISIS

Reviewed by JOSEPH KOCH, *Deutsche Literaturzeitung*, 1, 2418-20, 1930.

Hahn, Martin. Die Leibesübungen im mittelalterlichen Volksleben. 104 p. (*Friedrich Manns Pädagog. Magazin* H. 1260). Langensalza, BEYER & SÖHNE, 1929. ISIS

(Haskins, Charles Homer.) Anniversary essays in mediaeval history. By students of CHARLES HOMER HASKINS. Presented on his completion of forty years of teaching. Edited by CHARLES H. TAYLOR and JOHN L. LA MONTE. x+417 p., port. Boston, HOUGHTON MIFFLIN, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 433-36, 1930.

Hügli, Hilde. Der deutsche Bauer im Mittelalter dargestellt nach den deutschen literarisches Quellen vom 11-15 Jahrhundert. 176 p. Berlin, HAUPT, 1929. ISIS

Reviewed by JAMES WESTFALL THOMPSON, *Speculum*, 5, 227, 1930.

Hulme, Edward Maslin. The Middle Ages. x+851 p. New York, HOLT, 1929. ISIS

Reviewed by EDGAR H. MCNEAL, *American Historical Review*, 35, 322, 1930.

Krauss, Werner. Das tätige Leben und die Literatur im mittelalterlichen Spanien. 96 p. Stuttgart, KOHLHAMMER, 1929. ISIS

Reviewed by AMÉRICO CASTRO, *Deutsche Literaturzeitung*, 1, 2175-80, 1930.

Lasteyrie, Robert Charles de. L'architecture religieuse en France à l'époque romane. Seconde édition revue et augmentée d'une bibliographie critique, par MARCEL AUBERT. x+857 p. Paris, PICARD, 1926. ISIS

Reviewed by L. H. LABANDE, *Journal des Savants*, 13-22, 1930.

Laux, Rudolf. Ars medicinae. Ein frühmittelalterliches Kompendium der Medizin. *Kyklos*, 3, 417-34, 1930. ISIS

« Der Ausgangspunkt der vorliegenden Arbeit ist die Kopenhagener Handschrift Gl. Kgl. S. 1653 (H), Saec. XI, die fol. 72*r*-76*v* unter dem Titel *Ars medicinae* eine Sammlung von Traktaten, die keinen einheitlichen Charakter haben, enthält, die aber an die *Sapientia artis medicinae* erinnern, die M. WLASCHKY im *Kyklos* herausgegeben hat.» (*Isis*, 13, 208). « Inhaltlich reicher als diese Bearbeitung, bietet unsere Handschrift eine textliche Verwandtschaft und Übereinstimmung in weitem Masse mit den *Lecciones*

Heliodori, deren stark verstümmelter mythologischer Abschnitt erst durch Kenntnis der Kopenhagener Handschrift verständlich wird. Auch die drei restlichen Abschnitte der *Lecciones Heliodori* — Anatomie, Instrumentenliste, Passiones — erfahren wiederholt durch Vergleichung mit dem Kopenhagener Texte eine textliche Verbesserung. Während der Text von WLASCHKY sich in drei Teile gliedert, können wir in der Kopenhagener Handschrift neun Abschnitte unterscheiden. » « Es sind die folgenden : 1. Mythologische Einleitung; 2. HIPPOCRATES-vita; 3. Deontologisches; 4. Geschichte; 5. Anatomie; 6. Instrumentenliste; 7. Passiones; 8. Epistulae HIPPOCRATIS; 9. Lunaris cursus. » Critical edition of the text with brief study, and bibliography. G. S.

Lloyd, John Edward. The Welsh chronicles : the Sir JOHN RHYS Memorial lecture, British Academy, 1928. 25 p. (From the *Proceedings of the British Academy*, vol. 14). London, Oxford University Press, 1928. ISIS

Longnon, Jean. Les Français d'Outre-Mer au moyen âge : essai sur l'expansion française dans le bassin de la Méditerranée. Deuxième édition. IV+334 p. Paris, PERRIN, 1929. ISIS

Reviewed by D. C. MUNRO, *American Historical Review*, 35, 655, 1930.

Lowe, Elias Avery. Scriptura Beneventana : facsimiles of South Italian and Dalmatian manuscripts from the sixth to the fourteenth century. Oxford, Clarendon Press, 1929. ISIS

Maurer, Friedrich. Studien zur mitteldeutschen Bibelübersetzung vor LUTHER. XII+144 p. (*German. Bibl.* begr. v. WILH. STREITBERG. 2 Abt. : Untersuchungen u. Texte. Bd. 26). Heidelberg, WINTER, 1929. ISIS

Reviewed by GUSTAV EHRISMANN, *Deutsche Literaturzeitung*, 1, 2277-78, 1930.

Mayer, Claudius Franz. Das Zeitalter der Mönchs- und Priestermedizin in Ungarn. *Kyklos*, 3, 354-94, 1930. ISIS

« Die geschichtliche Periode der Mönchsmedizin nimmt ihren Anfang in Ungarn erst im 11. und dauert fast bis zum 16. Jahrhundert, bis tüchtige ausländische Laienärzte, vor allem Deutsche und Italiener, die ihre Ausbildung an Hochschulen erworben hatten, schon in genügender Zahl vorhanden waren und so die heilende Hilfe der Klöster immer weniger in Anspruch genommen wurde ». Elaborate study, including brief biographies of 29 Hungarian physicians of that period. G. S.

Mély, Fernand de. Les primitifs et leurs signatures. Nos vieilles basiliques et leurs maitres d'œuvre. Deux volumes. 250 p., 70 pl. Paris, GEUTHNER, 1929. ISIS

Muller, Henri François. A chronology of vulgar Latin. IX+172 p. (*Beih. z. Zeitschr. f. Roman. Philol.* begr. v. GUST. GRÖBER. fortgef. u. hrsg. v. ALF. HILKA. H. 78). Halle, NIEMEYER, 1929. ISIS

Reviewed by W. MEYER-LÜBKE, *Deutsche Literaturzeitung*, 1, 1982-86, 1930.

Puhlmann, Walter. Die lateinische medizinische Literatur des frühen Mittelalters. Ein bibliographischer Versuch. *Kyklos*, 3, 395-416, 1930. ISIS

Ricci, Seymour de. A list of medieval manuscripts in the New York Public Library. 28 p. New York, Public Library, 1930. ISIS

Richer, Paul. Nouvelle anatomie artistique. Le nu dans l'art. L'art chrétien depuis les origines jusqu'à la Renaissance. 246 p., 257 fig. Paris, PLON, 1929. ISIS

Reviewed by HENRY LEMONNIER, *Journal des Savants*, 433-441, 2 fig., 1929.

Schneider, Hermann. Die Kulturleistungen der Menschheit. 2. Bd. 1. Abtlg. Die Kulturen der Altfranzosen und der Altdeutschen. 224 p. Leipzig, WEBER, 1927. ISIS

Reviewed by SUDHOFF, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 29, 163-64, 1930.

Sigerist, Henry E. Masse und Gewichte in den medizinischen Texten des frühen Mittelalters. *Kyklos*, 3, 439-44, 1930. ISIS

« Jedem, der mit mittelalterlichen medizinischen Texten gearbeitet hat, werden die in den Rezepten angegebenen Masse und Gewichte schon Schwierigkeiten bereitet haben. Und zwar in zweifacher Hinsicht. Einmal sachlich, indem man sich immer überlegen muss, was für ein System zugrunde gelegt ist und welchen heutigen Massen und Gewichten die mittelalterlichen entsprechen. Dann sind aber auch noch äussere Schwierigkeiten, indem viele Abkürzungen in den üblichen Nachschlagebüchern (wie z. B. CAPPELLI) vergeblich gesucht werden. Diese Schwierigkeiten empfinden nicht nur wir. Auch den mittelalterlichen Kompilatoren haben sie manches Kopfzerbrechen bereitet, und wir finden daher in zahlreichen medizinischen Handschriften des frühen Mittelalters kurze Traktate *De ponderibus et mensuris* und bisweilen auch Verzeichnisse der Abkürzungen. Diese Traktate werden zur Zeit in unserem Institut gesammelt und sollen einmal im Zusammenhang veröffentlicht werden. Schon heute lässt sich sagen, dass die meisten von ihnen auf die Etymologien des ISIDOR VON SEVILLA zurückgehen. Die Kapitel 25 und 26 von Buch XVI haben wie so manche Abschnitte des Isidorschen Werkes ein Sonderleben geführt. Da sie für die medizinische Literatur des frühen Mittelalters massgebend waren, möchte ich sie hier nach der Ausgabe von W. M. LINDSAY abdrucken ».

Stadelmann, Rudolf. Vom Geist des ausgehenden Mittelalters. Studien zur Geschichte der Weltanschauung von NICOLAUS CUSANUS bis SEBASTIAN FRANCK. VIII+294 p. (*Dtsch. Vierteljahrsschrift f. Literaturwiss. u. Geistesgesch.* Hrsg. v. PAUL KLUCKHOHN u. ERICH ROTHACKER. Buchreihe Bd. 15). Halle, NIEMEYER, 1929. ISIS

Reviewed by ARTHUR HÜBNER, *DLZ*, 1, 54-60, 1930.

Strampf, Ilse von. Die Entstehung und mittelalterliche Entwicklung der Stadt Nürnberg in geographischer Betrachtung. *Sitzungsberichte der Physikalisch-medizinischen Sozietät zu Erlangen*, 60, 169-224, map, 1928. ISIS

Thorndike, Lynn. Prospectus for a corpus of medieval scientific literature in Latin. *Isis*, 14, 368-384, 1930. ISIS

Thorndike, Lynn. Seven salts of Hermes. *Isis*, 14, 187-88, 1930. ISIS

Apropos of a mediaeval text entitled *Secreta Hermetis regis Graecorum secundum transmutacionis naturam* (Brussels MS., XIIIth century). G. S.

Tyler, J. E. The Alpine passes. The Middle Ages (962-1250). 188 p. Oxford, BLACKWELL, 1930. ISIS

« Our main concern is with the use made of such routes across the Alps during a specific period, that between the coronation of OTTO I and the death of FREDERICK II (962-1250) and to investigate their importance, political and economic, at this time. This was not unnaturally considerable. The attempt of OTTO I and his successors to unite Italy and Germany under one sceptre, and the effort, too, made during and after the time of CONRAD THE SALIC, to add to this the government of Burgundy, endowed these passes with an importance greater, perhaps, than ever before or since. In these circumstances the Alpine pass-roads were links binding together a tripartite empire, factors which none of the emperors who sought to make their control over Germany, Italy and Burgundy a reality could afford to neglect. But their importance in this period was by no means exclusively political or strategic. These pass-roads were not simply, that is, military roads, not simply lines of communication between Germany and Italy or Italy and Burgundy, the most important of them at least, were, in addition, trade-routes. They provided a means for the commercial and industrial centres of Italy either to distribute goods which were needed north of the Alps, or to draw thence either raw materials, or articles destined for export from Italian sea-ports. The foundations of this traffic, which played such an important part in mediaeval commerce as a whole, were laid during the first half of the period with which we are concerned, and before 1250 much had been built on these foundations. The passes crossed by important roads during the period in question may be conveniently divided into three groups, the Western, the Central and Eastern respectively, as follows. *The*

Western group. The passes between the Col di Tenda and the Simplon inclusive; the most important of these being the Mont Genèvre, the Mont Cenis and the Great St. Bernard. *The Central group*. The passes from the Simplon up to, but not including the Reschen-Scheideck; the most important being the St. Gotthard, the Lukmanier and the Septimer. *The Eastern group*. The Reschen-Scheideck and those further east; the most important being the Brenner and the Pontebba. This subdivision, if convenient, is not entirely artificial. Those in the Western group all acted primarily as links between Italy and France; though one of them—the Great St. Bernard—was equally important for traffic between Italy and the Rhine below Basle. The passes in the Central group similarly linked Italy with the Rhine valley; those in the Eastern group Italy and the Danube. »

The main documents used by the author are (the date is quoted first, then the name of the traveler (or writer), then the pass) :

990. SIGERIC OF CANTERBURY. Great St.Bernard.

1154. NICHOLAS SEEMUNDARSON. Great St. Bernard.

1158. (told by MATTHEW PARIS). Mont Cenis.

1191. PHILIP AUGUSTUS. Mont Cenis.

1211. EMO, abbot of Floridus Hortus. Mont Cenis.

c. 1218. WOLFGER OF ELLENBRECHTSKIRCHEN. Pontebba, Brenner.

1236. ALBERT OF STADE. Mont Cenis, St. Gotthard, Brenner, Great St. Bernhard, Septimer.

« Though the Middle Ages lacked, unfortunately, the indefatigable zeal of a BAEDEKER, comparatively detailed accounts of five important pass-roads, the Mont Cenis, the Great St. Bernard, the St. Gotthard, the Brenner and Pontebba, are available. These, too, are records of the journeys of ordinary travellers, men likely, that is, to take the usual and best routes. » G. S.

Waddell, Helen. Mediaeval latin lyrics. VIII+352 p. London, CONSTABLE, 1929. ISIS

Walsh, James J. A golden treasury of medieval literature. XVI+314 p., 8 illus., Boston, STRATFORD, 1930. ISIS

No man has done more for the popularization of medieval thought than the author of this book. He has helped many thousands of Americans to appreciate mediaeval ideals. His latest book will perhaps be even more influential than the preceding ones. It is an anthology selected from the masterpieces of the Latin and vernacular literatures of Western Europe in mediaeval times : the Song of Roland, the Spanish epic ballads, the Arthurian legands, the Nibelungenlied, etc. Other chapters are entitled : The origin of rhyme in poetry; Great Latin hymns; Minnesingers, meistersingers; Troubadours, trouvères; Medieval women writers; Medieval books of manners; The three most read medieval books; Romance; « The jongleur of Notre Dame »; Gesta Romanorum; Philosophic prose; Didactic prose; History and biography; Books of travel; and the book ends most fittingly with an account of one of the greatest books of the world literature, the Imitation of Christ. The « three most read medieval books » are Reynard the Fox, the Golden Legend and the Romance of the Rose. G. S.

Zatschek, Heinz. Studien zur mittelalterlichen Urkundenlehre. Konzept, Register und Briefsammlung. X+146 p. (*Schriften d. philos. Fak. d. Dtsch. Univ. in Prag.* Bd. 4). Brünn, ROHRER, 1929. ISIS

Reviewed by WALTHER HOLTZMANN, *Deutsche Literaturzeitung*, 1, 1714-17, 1930.

Zinner, Ernst. Horologium viatorum. Anmerkungen zu den Mitteilungen von Herrn L. THORNDIKE in *Isis* Bd. 13, S. 51-93, 1929. *Isis*, 14, 385-87, 1930. ISIS

7. — BYZANTIUM

Freshfield, Edwin Hanson. A manual of Eastern Roman law. The Procheiros Nomos, published by the Emperor BASIL I at Constantinople, between 867 and 879 A.D., rendered into English. X+172 p. Cambridge, University Press, 1928. ISIS

Reviewed by C. A. SPULBER, *Byzantion*, 4, 575-82, 1929.

Jerphanion, Guillaume de. Une nouvelle province de l'art byzantin, les églises rupestres de Cappadoce. Texte, tome I (1re partie), LXIII+297 p., Planches, premier album, 69 pl. (Bibliothèque historique et archéologique du Haut Commissariat de la République Française en Syrie, 5). Paris, GEUTHNER, 1925. ISIS

Reviewed by LOUIS BRÉHIER, *Byzantion*, 3, 478-80, 1928.

Soyter, Gustav. Byzantinische Geschichtschreiber und Chronisten, ausgewählte Texte mit Einleitung, kritischem Apparat und Kommentar=Kommentierte griechische und lateinische Texte, herausgegeben von J. GEFFCKEN, 5, VIII+64 p. Heidelberg, WINTER, 1929. ISIS

Reviewed by HENRI GRÉGOIRE, *Byzantion*, 4, 744-46, 1929; by F. DÖLGER, *OLZ*, 33, 609, 1930.

Tafrali, O. Monuments byzantins de Curtéa de Argès. Texte et atlas. XXI+352 p.; XVI p., 157 pl. Paris, GEUTHNER, 1930. ISIS

« Les monuments byzantins de Curtéa de Argès présentent une importance particulière, tant pour l'histoire du peuple roumain et des Balkans, que pour l'art byzantin en général. Biserica-Donneasca (primatiale) est, de l'avis de tous, la plus ancienne église roumaine, bâtie au moment même de la création de l'État valaque. Lui assigner une date, c'est en fixer une pour la fondation de cet État, ce qui constitue le plus difficile problème que les historiens roumains ne sont jamais parvenus à résoudre d'une façon satisfaisante. Autour de cette question, on a longuement et ardemment discuté. Certains savants estiment que l'État valaque aurait été créé au XIIIe siècle; d'autres avancent cette date au commencement du XIVe siècle. » « Aux

faits historiques connus, commentés convenablement, l'auteur en apporte d'autres, puisés dans un certain nombre de textes. Il y ajoute les conclusions d'une étude approfondie sur l'architecture et la peinture de trois monuments byzantins de Curtéa de Argès, qu'il rapproche d'une riche série d'autres qui s'élèvent tant dans les Balkans qu'en Italie Méridionale. Il date Saint-Nicolas et les deux autres chapelles de Curtéa de Argès, du XIII[e] siècle. »

(Uspenskij, Théodore). L'art byzantin chez les Slaves. Les Balkans. Premier recueil dédié à la mémoire de THÉODORE USPENSKIJ. XV+504 p., 73 pl., 262 fig. (*Orient et Byzance*, études d'art médiéval publiées sous la direction de GABRIEL MILLET, IV). Paris, GEUTHNER, 1930. ISIS

Vasiliev, Aleksandr Aleksandrovich. History of the Byzantine Empire. Volume II, From the Crusades to the fall of the Empire (A.D. 1453). 502 p. Translated from the Russian by Mrs. S. RAGOZIN. (*University of Wisconsin Studies in the social sciences and history*, number 14). Madison, 1929. ISIS

Reviewed by ROBERT P. BLAKE, *American Historical Review*, 35, 904-905, 1930.

III. ORIENTAL SCIENCE AND CIVILIZATION

8. — ASIA

Western Asia

Moortgat, A. Der Kampf zu Wagen in der Kunst des alten Orients. Zur Herkunft eines Bildgedankens. *OLZ*, 33, 841-54, 3 pl., 1930 ISIS

Central Asia

David-Neel, Alexandra. Voyage d'une Parisienne à Lhassa. A pied et en mendiant de la Chine à l'Inde à travers le Thibet. 28 photographies hors texte et une carte. XII+332 p. Paris, PLON, 1927. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 441-44, 1930.

David-Neel, Alexandra. Mystiques et magiciens du Thibet. Préface de A. D'ARSONVAL. 25 gravures hors texte. VI+306 p. Paris, PLON, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 441-44, 1930.

Filchner, Wilhelm. Om mani padme hum. Meine China- und

Tibetexpedition 1925-28. 8. Aufl. IX+352 p., 103 fig., map. Leipzig, BROCKHAUS, 1930. ISIS

Reviewed by E. TRINKLER, *OLZ*, 33, 811-12, 1930.

Macdonald, David. The land of the Lama. A description of a country of contrasts and of its cheerful, happy-go-lucky people of hardy nature and curious customs, their religion, ways of living, trade and social life. With a foreword by the Earl of RONALDSHAY. 283 p., fig., map. London, SEELEY. 1929. ISIS

Reviewed by A. H. FRANCKE, *OLZ*, 33, 476-77, 1930.

Macdonald, D. Mœurs et coutumes des Thibétains. Traduction française par R. BILOT. *(Collection d'études, de documents et de témoignages pour servir à l'histoire de notre temps.)* 264 p. Paris, PAYOT, 1930. ISIS

Macmunn, Sir George. Afghanistan from DARIUS to AMANULLAH. XII+359 p., map. London, BELL, 1929. ISIS

Reviewed by A. CHRISTENSEN, *OLZ*, 915, 1930; in *Journal of the Royal Asiatic Society*, 174-75, 1931.

Roerich, Nicholas. Altai-Himalaya. A travel diary. XIX+407 p. London, JARROLDS, 1930. ISIS

Sleen, Wicher Gosen Nicolaas van der. Four months camping in the Himalayas. Translated by M. W. HOPER. XIII+213 p. London, ALLAN, 1929. ISIS

Reviewed by C. MABEL RICKMERS, *Journal of the Royal Asiatic Society*, 951, 1930.

Trinkler, Emil. Im Land der Stürme. Mit Yak- und Kamelkaravanen durch Innerasien. 242 p. Leipzig, BROCKHAUS, 1930. ISIS

Reviewed by C. MABEL RICKMERS, *Journal of the Royal Asiatic Society*, 205-07, 1931.

Eastern Asia

(Including works relative to the whole of Buddhist Asia, or to India, Central and Eastern Asia combined)

Grousset, René. Histoire de l'Extrême-Orient. 2 vols. X+769 p., 32 pl., frontispice, 7 cartes. Paris, GEUTHNER, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 437-41, 1930.

Wheeler, Leonhard Richmond. The modern Malay. 300 p. London, ALLEN & UNWIN, 1928. ISIS

Reviewed by H. JENSEN, *OLZ*, 33, 928-29, 1930.

9. — INDIA

Bachhofer, Ludwig. Early Indian sculpture. 2 vol. XXXVI+137 p., 161 pl. New York, Pegasus Press, 1929. ISIS

Reviewed by ETHEL HAHN, *The Art Bulletin*, 12, 201-02, 1930.

Das, Sukumar Ranjan. Some notes on Indian astronomy. *Isis*, 14, 388-402, 1930. ISIS

Vindication of the originality and antiquity of Hindu astronomy. G. S.

Datta, Bibhutibhusan. Geometry in the Jaina cosmography. *Quellen und Studien zur Geschichte der Mathematik*. Abteilung B : Studien, 1, 245-54, 1930. ISIS

« In the cosmography of the Jainas are found applications of certain geometrical, or rather mensuration formulae. Some of them pertaining to the geometry of circle have already been noted in my article on ' The Jaina School of Mathematics '. There are also others as regard the theory of proportional triangles and the area of the segment of a circle. It is desirable that all of them should be dealt with together at one place so as to give a fairly accurate idea of the extent of the knowledge of geometry amongst the early Jainas. » « The sources on which we have mainly relied for information about the knowledge of the science of geometry amongst the early Jainas are unfortunately very uncertain as regards their dates. The earliest canonical works whose authorship is traditionally attributed to MAHĀVĪRA (died 527 B.C.), the great founder of the Jainism, are referred by modern scholars to the period 500-300 B.C. The same uncertainty exists about the time of UMĀSVĀTI. For reasons indicated elsewhere, we have placed him about the middle of the second century B.C. But he may be posterior by a century or two. JINABHADRA GAṆI lived in the years 529-589 A.D. Thus our sources cover a period extending over a thousand years. We have not referred to cosmographical works of later times as they are merely compilations from the earlier ones. To recapitulate : in the earlier period (500-300 B.C.) the Jainas had a certain amount—a fair amount indeed—of the knowledge of the geometry of circles, circular segments and proportional triangles. In the succeeding period though we find a clear and explicit formulation of those properties, the Jainas added practically nothing new to their stock of knowledge of geometry. Towards the latter part of the period attempts were made to determine the area of a circular segment and the volume of certain solids. But all those ended in failure. Only a few very roughly approximate formulae were obtained. But most of them were discarded by the mathematicians. For they do not appear in the Gaṇita-sārasaṁgraha (Collection of the essence of Mathematics) of MAHĀVĪRA (c. 850 A.D.). »

Datta, Bibhutibhusan. Origin and history of the Hindu names for geometry. *Quellen und Studien zur Geschichte der Mathematik.* Abteilung B : Studien, 1, 113-19, 1930. ISIS

Jacobi, Hermann. Beiträge zur Literaturwissenschaft und Geistesgeschichte Indiens. Festgabe HERMANN JACOBI zum 75. Geburtstag (11. Febr. 1925) dargebracht von Freunden, Kollegen und Schülern, hrsg. von WILLIBALD KIRFEL. VI+460 p. Bonn, KLOPP, 1926. ISIS

Reviewed by PAUL THIEME, *OLZ*, 33, 546-49, 1930.

Jolly, Julius. Hindu law and custom. Authorized translation by BATAKRISHNA GHOSH. XI+341 p., VII-I p. (*Greater India Society* Publication no. 2). Calcutta, 1928. ISIS

Reviewed by L. D. BARNETT, *Journal of the Royal Asiatic Society*, 930-32, 1930.

Karandikar, S. V. Hindu exogamy. XV+308 p. Bombay, TARAPOREVALA, 1929. ISIS

Reviewed by R. E. E., *Journal of the Royal Asiatic Society*, 946-48, 1930.

Krishnaswami, G. V. Reform of the Indian calendars. *Isis*, 14, 403-10, 1930. ISIS

« The calculations of the Indian calendars have been discussed by various writers from the chronological point of view; in this paper I deal with the main features of the Indian calendars and examine their agreement with observed facts. The Hindu theory of the precession of the equinoxes and the terms sayana and mirayana are explained. The meanings of the terms year, ayana, month, tithi and nakashatra are considered and it is shown how two kinds of years, months, etc. (tropical, sidereal) are possible but that there can be only one interpretation for ayana. The method of placing the ayanas and the months on a sidereal basis in the Indian calendars is defective and hence there is a difference of about 23 days in the calculations, which difference will increase in course of time. The equinoxes and solstices will therefore occur at varying parts of the year and there will be a corresponding acceleration of seasonal changes. One important function of the calendar is the regulation of religious and sacrificial rites in accordance with the Shastras, and this function fails if the sidereal calendar is followed. I make a few suggestions to bring the Indian Calendars into line with actual observations. »

Macdonell, Arthur Anthony. India's past. A survey of her literatures, religions, languages and antiquities. XII+293 p., 35 illus., 4 maps. Oxford, Clarendon Press, 1927. ISIS

Reviewed by O. STEIN, *OLZ*, 33, 792-94, 1930.

Meyer, Johann Jakob. Gesetzbuch und Purāna. Ein Beitrag zur Frage von der Entstehungsart der altindischen Rechtsschriften und

der Purāna. XIII+112 p. (*Indische Forschungen*, 7). Breslau, MARCUS, 1929. ISIS

Reviewed by H. LOSCH, *OLZ*, 33, 796-802, 1930.

Mukhopadhyaya, Pramathanath. Introduction to Vedanta philosophy. VII+259 p. (Sreegopal Basu Mallik Fellowship lectures for 1927). Calcutta, The Book Co., 1928. ISIS

Reviewed by TARACHAND ROY, *OLZ*, 33, 920-23, 1930.

Râdhakrishnan, Sarvepalli. The Vedânta according to SAMKARA and RÂMÂNUJA. 287 p. London, ALLEN & UNWIN, 1928. ISIS

Reviewed by W. RUBEN, *OLZ*, 33, 543-45, 1930.

Renou, Louis. Bibliographie Védique. 350 p. Paris, MAISONNEUVE, 1931. ISIS

Rosenthal, Ethel. The story of Indian music and its instruments. A study of the present and a record of the past. Together with Sir WILLIAM JONES' celebrated treatise in full (1784). XXVII+220 p., 19 pl., map. London, WILLIAM REEVES, 1929. ISIS

Reviewed by B. BRELOER, *OLZ*, 34, 67-9, 1931.

Sköld, Hannes. Untersuchungen zur Genesis der altindischen etymologischen Literatur. 103 p. (*Lunds Universitets Årsskrift*, N.F. Avd. 1, Bd. 24, Nr. 6). Lund, GLEERUP, 1928. ISIS

Reviewed by P. THIEME, *OLZ*, 33, 1032-37, 1930.

Suvimala Chandra Sarkâr. Some aspects of the earliest social history of India (pre-Buddhistic ages). XVII+225 p. London, Oxford University Press, 1928. ISIS

Reviewed by RICHARD SCHMIDT, *OLZ*, 34, 63, 1931.

Winternitz, Moriz. Der ältere Buddhismus nach Texten des Tipitaka. VI+162 p. (*Religionsgeschichtl. Lesebuch*, in Verbind. mit Fachgelehrten hrsg. von ALFRED BERTHOLET. 2., erw. Aufl. Heft 11). Tübingen, MOHR, 1929. ISIS

Reviewed by J. WITTE, *OLZ*, 33, 1040, 1930.

10. — CHINA

Burgess, John Stewart. The guilds of Peking. 270 p. New York, Columbia Univ. Press, 1928. ISIS

Reviewed by ERICH HAUER, *OLZ*, 33, 553-55, 1930.

Buxton, L. H. Dudley. China, the land and the people. A human

geography. With a chapter on the climate by W. G. KENDREW. XIII+333 p., maps, figs. Oxford, Clarendon Press, 1929. ISIS

Reviewed by BERNHARD KARLGREN, *Deutsche Literaturzeitung*, 2, 12-14, 1930; by F. E. A. KRAUSE, *OLZ*, 33, 552, 1930.

Creel, Herrlee Glessner. Sinism. A study of the evolution of the Chinese world-view. IX+127 p. Chicago, Open Court, 1929. ISIS

Reviewed by F. E. A. 'KRAUSE, *OLZ*, 34, 174-76, 1931.

Granet, Marcel. Chinese civilization. Translated by KATHLEEN E. INNES and MABEL R. BRAILSFORD. XXIII+444 p., 12 pl. (The History of Civilization Series.) London, KEGAN PAUL, 1930. ISIS

Reviewed in *Nature*, 127, 88, 1931.

Granet, Marcel. La civilisation chinoise. La vie publique et la vie privée. XXI+523 p.; 10 pl., 5 maps. (L'évolution de l'humanité). Paris, La Renaissance du Livre, 1929. ISIS

Reviewed by A. FORKE, *OLZ*, 33, 931-35, 1930.

Hentze, C. Le poisson comme symbole de fécondité dans la Chine ancienne. *Bull. des musées royaux*, 2, 141-52, 12 fig., Bruxelles, 1930. ISIS

Hübotter, Franz. Die chinesische Medizin zu Beginn des XX. Jahrhunderts und ihr historischer Entwicklungsgang. Leipzig, Asia major, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 255-63, 2 fig., 1930.

Hummel, Arthur W. Division of Chinese literature. Chinese and other east Asiatic books added to the Library of Congress, 1929-30. The Library of Congress, Report for 1929-30, p. 341-68, Washington, 1931. ISIS

For the previous report see *Isis* 14, 525. « The number of Chinese works received during the fiscal year was 410, comprising 3,085 volumes, not including some 414 monographs and pamphlets. The additions in Japanese number 56 works in 149 volumes; those in Tibetan number 550 volumes, of which 543 are Nashi pictographic manuscripts brought to this country by Dr. JOSEPH ROCK and described on page 386 of this report. On July 1, 1930, the total number of volumes in the Chinese collection was 136, 772 and in the Japanese 12,158. In addition, the Library has several thousand volumes in Korean, Manchurian, Mongolian, and Tibetan. » The main acquisitions are briefly described. G. S.

Rudd, Herbert F. Chinese social origins. IX+221 p. Chicago, University of Chicago Press, 1928. ISIS

Reviewed by ERICH HAUER, *OLZ*, 33, 310-11, 1930.

Swingle, Walter T. Notes on Chinese accessions on medicine and materia medica, and on Nashi pictographic manuscripts. The Library of Congress, Report for 1929-30, p. 368-90. Washington, 1931. ISIS

« In the report for 1924 notice was made of the acquisition through the good offices of Dr. JOSEPH F. ROCK of a large collection of pictographic manuscript books of the so-called Moso or Nashi people of Yünnan. In April, 1930, Doctor ROCK again returned to Washington after several years' work for the National Geographic Society in the least-known parts of west China and eastern Tibet, bringing with him for the Library of Congress another and much larger collection of Nashi pictographic books. The collection noticed in the report for 1924, consisting of 69 volumes, was the largest then known in Europe or America, but the last collection brought from Yünnan by Doctor ROCK comprises no fewer than 542 items (529 manuscript volumes and 13 charts and pictures). This collection was secured by Doctor ROCK from the Nashi tombas or priests after he had lived for some years among the Nashi people and had learned to speak their language. The tombas estimate their total literature at about 1,600 volumes, so this last acquisition represents about one-third of all the Nashi literature extant. Together with the items secured in 1924 the Library of Congress now has nearly 600 Nashi manuscript volumes, far and away larger than any other single collection known. »

« Most of these books concern religious ceremonies, many of them long and highly complicated. Some of the books, however, are historical in character and some few are dramas that are highly prized by the Nashi people. »

« A number of the Nashi books secured by Doctor ROCK relate to divination, and one of these books secured at Yung-ning, No. 524, Ba-ba-ba-lö-la-zur, used to determine whether smallpox appears on lucky or unlucky days, has scientific interest, since it describes the inoculation of children with smallpox to render them immune to the disease. The Nashi literature is recorded in what is probably the only pictographic writing in use to-day on a fairly large scale by a semi-independent race. It should be noted that the Nashi people has, to a surprising degree, preserved its customs and religion in spite of age-long contacts with the Tibetans on the one hand and the Chinese on the other, and also in spite of the penetration of European missionaries. »

Uyeda, Minoru. Sekishi seikyō no kenkyū (Studies in the star catalogue of Shih Shên. IV+IV+184 p., 4 pl. Tōyō Bunka, Tōkyō, 1930. (published in Japanese, Jan. 1931 ; but dated 1930.) ISIS

This star catalogue is of great interest because it represents observations made in the fourth century B.C. The work is provided with a preface by Dr. SHINZŌ SHINJŌ, President of the Kyōto Imperial University, who has a deep knowledge of ancient Chinese astronomy. Y. M.

Wittfogel, K. A. Wirtschaft und Gesellschaft Chinas. XVIII+768 p., ill. Leipzig, HARRASSOWITZ, 1930. ISIS

11. — JAPAN

Hannack, Georg. Japanischer Damaststahl. *Beiträge zur Geschichte der Technik und Industrie*, 20, 87-90, 28 fig., 1930. ISIS

Ikeda, Shingo. Vom japanischen Badeleben. Bemerkungen zu einem Farbenholzschnitt von UWOYA HOKKEL. *Kyklos*, 3, 435-38, pl., 1930. ISIS

Lombard, Frank Alanson. An outline history of the Japanese drama. With an introduction by GEORGE PIERCE BAKER. 358 p. London, ALLEN & UNWIN, 1928. ISIS

Reviewed by M. RAMMING, *OLZ*, 33, 74, 1930.

Stopes, Marie Charlotte. Plays of old Japan. The « Nô ». Together with translations of the dramas by M. C. STOPES and JOJI SAKURAI, with a preface by Baron KATO. Second ed., facsimile of the first. Illustrated. VII+104 p. London, Eclipse Press, 1927. ISIS

Reviewed by A. CHANOCH, *OLZ*, 33, 490-92, 1930.

Takagi, K. Nihon chizu-sokuryō shōshi (Short history of Japanese maps and surveying). XVI+170+XV p., Tōkyō, KOKON SHOIN, 1931 . ISIS

Brief but interesting account. Y. M.

12. — ISRAEL

Browne, Laurence Edward. Early Judaism. XIV+234 p. Cambridge, University Press, 1929. ISIS

Reviewed by GEORGE BEER, *OLZ*, 33, 1002-03, 1930.

Causse, A. Les dispersés d'Israël. Les origines de la diaspora et son rôle dans la formation du Judaïsme. 166 p. (*Etudes d'histoire et de philosophie religieuses* publiées par la Faculté de Théologie Protestante de l'Université de Strasbourg, Nr. 19). Paris, ALCAN, 1929. ISIS

Reviewed by CURT KUHL, *OLZ*, 34, 49-51, 1931.

Davidson, Israel. Thesaurus of medieval Hebrew poetry. Vol. 2. XIII+490 p. New York, Jewish Theological Seminary of America, 1929. ISIS

Fiebig, Paul. Rabbinische Gleichnisse. Vokalisierte hebräische und

aramäische Texte, dargeboten für das Studium der Gleichnisse JESU mit Verzeichnis der nichtbiblischen Wörter. IV+51 p. Leipzig, HINRICHS, 1929. ISIS

Reviewed by G. DALMAN, *OLZ*, 296, 1930.

Gandz, Solomon. The knot in Hebrew literature, or from the knot to the alphabet. *Isis*, 14, 189-214, 1930. ISIS

I. Introductory. 1. The natural and the mechanical memory. 2. Writing, the memory-machine. 3. Knots, the primitive memory-tools. II. Knots in magic and superstition. 4. Magic knots. 5. The magic knot in Hebrew literature. 6. The cameo. 7. The vow. Binding and loosing. 8. The magic knot in the Qurân. 9. The one who loosens the knot. III. Historical and mnemonic knots. 10. The Quipu. 11. Two aspects of the Quipu. 12. The phylacteries (tephillin). 13. The fringes (ṣiṣith). 14. More evidence for mnemonic knots in the Bible. 15. The rôle of the Quipu in the organization of revolts. IV. Mathematical knots. 16. Knots in counting. 17. The knot-abacus. 18. Arabic, Roman, and Greek traces of the knot-abacus. Articuli. 19. Knotted cords as registers and accounts. 20. The publican's knot in Palestine. V. Knot-letters or knot-numerals. 21. Knot-receipts. 22. The Ghobar-numerals, or apices, are called « Roman knots ». 23. Knot-letters in the phylacteries. 24. Egyptian and Chinese knot-letters. 25. Conclusion. From the knot to the alphabet.

Geiger, Abraham. Urschrift und Übersetzungen der Bibel in ihrer Abhängigkeit von der inneren Entwicklung des Judentums. 2. Aufl. mit einer Einführg. von P. KAHLE und einem Anhang, enthaltend : Nachträge zur Urschrift, Verzeichnis der Bibelstellen und Bibliographie, zusammengestellt u. bearb. von N. CZORTKOWSKI. VIII+500+51 p. Frankfurt a.M., WAHRMANN, 1928. ISIS

Reviewed by M. GRÜNEWALD, *OLZ*, 33, 1000-02, 1930.

Goldschmidt, Lazarus. Der babylonische Talmud. Nach der ersten zensurfreien Ausgabe unter Berücksichtigung der neueren Ausgaben und handschriftlichen Materials neu übertragen. Bd. I : Berakhoth, Mishna Zeraim, Shabbath. XV+947 p. Berlin, Biblion, 1929. ISIS

Reviewed by PAUL FIEBIG, *OLZ*, 33, 363, 1930.

Higger, Michael. Aggadot Tannaim. A collection of all the aggadic material in the Tosefta arranged and edited with a complete index of subject matter. 168 p. New York, BLOCH, 1928. ISIS

Reviewed by W. WINDFUHR, *OLZ*, 33, 200-01, 1930.

Hildesheimer, Esriel Erich. Das jüdische Gesellschaftsrecht. XVI+172 p. Leipzig, HARRASSOWITZ, 1930. ISIS

Jensen, Peter. Das Gilgamesh-Epos in der Weltliteratur. Bd. 2: Die israelitischen Gilgamesch-Sagen in den Sagen der Weltliteratur. Mit einem Ergänzungsheft, worin unter anderem 4 Kapitel über die Paulus-Sage. Lfg. 1-3. XVI+165+732 p. Marburg, EBEL, 1928-29. ISIS

Reviewed by WALTER ANDERSON, *Deutsche Literaturzeitung*, 1, 1887-90, 1930.

Kleinman, Moses. Demuyot we-komot (portraits and personalities). Essays on modern Hebrew literature. 336 p. London, MAZIN, 1930. ISIS

Obermayer, Jacob. Die Landschaft Babylonien im Zeitalter des Talmuds und des Gaonats. Geographie und Geschichte nach talmudischen, arabischen und anderen Quellen. VIII+362 p., 2 maps. (*Schriften der Gesellschaft zur Förderung der Wissenschaft des Judentums*, Nr. 30). Frankfurt a.M., KAUFFMANN, 1929. ISIS

Reviewed by S. KLEIN, *OLZ*, 33, 451-54, 1930.

Rosen, Georg. Juden und Phönizier. Das antike Judentum als Missions-Religion und die Entstehung der jüdischen Diaspora. Neu bearbeitet und erweitert von FRIEDRICH ROSEN und D. GEORG BERTRAM. VIII+185 p. Tübingen, MOHR, 1929. ISIS

Reviewed by LOUIS FINKELSTEIN, *The Jewish Quarterly Review*, 21, 330-31, 1931.

(Rosenbaum, M.; Silbermann, A. M.; Blashki, A.; Joseph, L.) Pentateuch with targum Onkelos, haphtoroth and prayers for Sabbath and Rabbi's commentary, translated into English and annotated by M. ROSENBAUM and A. N. SILBERMANN in collaboration with A. BLASHKI and L. JOSEPH. Genesis. 281 p.-61. London, SHAPIRO, VALLENTINE, 1929. ISIS

Reviewed by M. GASTER, *Journal of the Royal Asiatic Society*, 906-09, 1930.

Schaefer, Moritz. Festschrift zum 70. Geburtstage von MORITZ SCHAEFER. Zum 21. V. 1927 hrsg. von Freunden und Schülern. XVI+273 p., 1 pl. Berlin, Philo-Verlag, 1927. ISIS

Reviewed by MAX GRÜNEWALD, *OLZ*, 33, 124-26, 1930.

Schechter, Solomon. (-1915). Genizah studies in memory of SOLOMON SCHECHTER. I. Midrash and Haggadah by LOUIS GINZBERG. XVII+549 p. II. Geonic and early Karaitic Halakah by LOUIS GINZBERG. XV+640 p. III. Liturgical and secular poetry

by ISRAEL DAVIDSON. XIV+338 p. (*Texts and Studies of the Jewish Theological Seminary of America*, vol. VII, VIII, IX), New York, Jewish Theological Seminary of America, 1928. ISIS

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Reviewed by CURT KUHL, *OLZ*, 33, 998-999, 1930.

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Reviewed by G. SCHOLEM, *OLZ*, 33, 1009, 1930.

Sobhy, G. P. G. The book of the Proverbs of SOLOMON in the dialect of Upper Egypt. Cairo, University of Egypt, 1929. ISIS

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13. — IRAN

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Reviewed by FR. ROSEN, *OLZ*, 33, 917, 1930.

Pope, Arthur Upham. An introduction to Persian art since the seventh century A.D. With more than a hundred illustrations, a bibliography, a map of Persia and a chronological table. London, DAVIES, 1930. ISIS

Rogers, Robert William (1864-1930). A history of ancient Persia from its earliest beginnings to the death of ALEXANDER THE GREAT. XV+393 p., ills., maps. New York, SCRIBNERS', 1929. ISIS

Reviewed by A. CHRISTENSEN, *OLZ*, 914, 1930.

Sakisian, Arménag Bey. La miniature persane du XII^e^ au XVII^e^ siècle. 174 p., 191 fig. Paris, VAN OEST, 1929. ISIS

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Sykes, Sir Percy. A history of Persia. Third edition with supplementary essays. Vol. 1. XXXIX+563 p., 15 pl. Vol. 2. XX+616 p., 16 pl. London, MACMILLAN, 1930. ISIS

Reviewed by A. T. W., *Nature*, 126, 871-72, 1930.

Wesendonk, O. G. von. The Kâlavâda and the Zervanite system. *Journal of the Royal Asiatic Society*, 53-109, 1931. ISIS

« The Zervanite theology of Iran, once the official system of the Persian empire, and the Indian Kâlavâda have in common the acceptation of time as the supreme principle and the connection of this notion with the idea of fate. This leads to astrological considerations, some of which are in contrast with the Indian conception of Karman, as they subject the human destiny to a necessity lying outside the sphere of man's action and mind... »
« There seems to exist no contact between the Iranian Zervanites and the Kâlavâda. The Indian development can be fully understood from Indian premises alone. One point remains however dubious. Did India discover by herself the notion of absolute time or did it come to her perhaps through Iran from Greece? The question cannot be separated from that of the origin of Indian astrology. For Iran it seems clear that Zervanism was a result of the Hellenistic current, forming a new world out of Eastern and Greek elements, a movement which need not be limited to the period of ALEXANDER, but which goes back to the days when the Achaemenian empire came into touch with the Western world. »

14. — ISLAM

Castagné, J. Magie et exorcisme chez les Kazak-Kirghizes et autres peuples Turks orientaux. *Revue des études islamiques*, 1, 53-156, 2 pl., 1930. ISIS

Chapter II (p. 125-56) deals with popular medicine. G. S.

Farmer, Henry George. The origin of the Arabian lute and rebec. *Journal of the Royal Asiatic Society*, 767-83, 1930. ISIS

Filchner, Wilhelm. Hui-Hui. Asiens Islam-Kämpfe. 423 p., 18 ill., map. port. Berlin, OESTERGAARD, 1928. ISIS

Reviewed by K. HAUSHOFER, *OLZ*, 33, 930, 1930.

Goichon, A. M. La vie féminine au Mzab. Notes complémentaires à l'étude publiée sous le même nom en 1927. *Revue des études islamiques*, 231-87, 1930. ISIS

Lane-Poole, Stanley. The Mohammedan dynasties. Turkish translation with additions and corrections by KHALIL EDHEM. Published by the Turkish Ministry of education, Istanbul, 1927. ISIS

First printed in English, London, 1893. Photographic reprint, Paris, 1925 (*Ins*, 8, 794). Elaborate review of the Turkish edition by K. SÜSSHEIM, *OLZ*, 33, 252-74, 1930.

Meyerhof, Max. Von Alexandrien nach Bagdad. Ein Beitrag zur Geschichte des philosophischen und medizinischen Unterrichts bei

den Arabern. 43 p. (Sonderausgabe aus den *Sitzungsberichten der Preussischen Akademie der Wissenschaften, Phil.-Hist. Klasse*, XXIII, 1930). Berlin, DE GRUYTER, 1930. ISIS

« Es ist bekannt, dass das Studium der Medizin und der Philosophie, insbesondere das des hippokratisch-galenischen Kanons und des aristotelischen Organons, schon lange vor dem Auftreten des Islams von Alexandrien aus in das sasanidisch-persische Reich eingedrungen war und auf Grundlage syrischer und arabischer Übersetzungen vom VIII. Jahrhundert u. Z. an festen Boden im Chalifenreiche gewonnen hat. Dagegen sind bisher diejenigen arabischen Quellenwerke nicht hinreichend beachtet worden, welche von einer direkten Wanderung des scholastischen Unterrichts, hauptsächlich in der Logik, von Alexandrien über Antiochien und Ḥarrān nach Baghdad in der Zeit von etwa 720 bis 900 n. Chr. berichten. Diese Nachrichten gehen zumeist auf AL-FĀRĀBĪ zurück, der sie von seinen christlichen Lehrern erhalten haben muss. Doch waren auch ihm die Namen der Schulhäupter in Alexandrien und Antiochien schon nicht mehr bekannt. Vielleicht kann diese Lücke in der Wissenschaftsgeschichte noch in Zukunft aus syrischen Dokumenten einmal ausgefüllt werden. Für die Zeit von etwa 850 bis 1100 n. Chr. ist die Tradition der Schulhäupter und bedeutenderen Lehrer der Philosophie und Medizin ziemlich lückenlos. »

Musil, Alois. The manners and customs of the Rwala Bedouins. *American Geographical Society, Oriental explorations and studies* no. 6. Edited by J. K. WRIGHT. XIV+712 p., 59 fig. New York, American Geographical Society, 1928. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 444-46, 1930.

Plessner, Martin. Arabische Alchemie im lateinischen Abendlande. *OLZ*, 33, 721-27, 1930. ISIS

Apropos of Mrs. D. W. SINGER's Catalogue of alchemical MSS. (vol. 1, Brussels, 1928; *Isis*, 12, 168-69). G. S.

Reitzenstein, R. Heilige Handlung. *Bibliothek Warburg Vorträge*, 1928-1929, 21-41, 1930. ISIS

Schaade, A. AHMED TAIMÛR PASHA (1871-1930) und die arabische Renaissance. *OLZ*, 33, 854-59, 1930. ISIS

Schmitz, Fritz. Orientalischer Damaststahl. *Beiträge zur Geschichte der Technik und Industrie*, 20, 81-86, 5 fig., 1930. ISIS

« Der Verfasser ist der Ansicht, dass die orientalischen Damaststahlklingen und wahrscheinlich auch die meisten andern Klingen des Altertums und des Mittelalters eine Abschreckung unter Martensitbildung nicht erhalten haben. Vielmehr glaubt er, dass die Härte dieser Klingen durch Kaltschmieden erzeugt wurde, nachdem diese vorher in Dunkelrotglut verarbeitet worden sind. Entsprechend dieser Annahme ist das Gefüge der Damaszenerklingen

als kalt bearbeiteter Sorbit mit sehr stark zertrümmerten Zementiteinschüssen zu deuten, was durch eigene Schmiedeversuche des Verfassers bestätigt wird. Die durch eine derartige Verarbeitung der Klingen entstandenen Eigenschaften sind hohe Streckgränze, Festigkeit, Elastizität, Härte und Zähigkeit. Seine Annahme, dass im Altertum und wahrscheinlich auch im Mittelalter die Härtung von Klingen verschiedenster Art durch Kaltbearbeitung als etwas Gebräuchliches anzusehen ist, wird durch Angaben des älteren Schrifttums in einwandfreier Weise belegt.»

Seemann, H. J. EILHARD WIEDEMANN (geb. 1. August, 1852, gest. 7 Januar 1928). *Isis*, 14, 166-86, pl., 1930. ISIS

Biography with bibliography and portrait. G. S.

Seippel, Alexander. Rerum Normannicarum fontes Arabici. Collegit et edidit ALEXANDER SEIPPEL. 45+154+LXXIV p. Oslo, 1896-1928. ISIS

Reviewed by D. S. M., *Journal of the Royal Asiatic Society*, 155-57, 1931.

Ṭâhâ Ḥusain; Ahmad Amîn; 'Abdalḥamîd al-'Abbâdî. Fajr al-Islâm (The dawn of Islam). Part 1: AḤMAD AMÎN: Filḥayât al-'aqlîya (Intellectual life). XVIII+355 p. Cairo, Lajnat at-ta'lîf wat-tarjama wan-nashr, 1928. ISIS

Reviewed by JOSEPH SCHACHT, *OLZ*, 34, 51-3, 1931.

Thorndike, Lynn; Sarton, George. On the use of the word kardaja. (Query no. 14, answer no. 14). *Isis*, 14, 420-22, 1930. ISIS

Titus, Murray T. Indian Islam: a religious history of Islam in India. XVI+290 p. (*The Religious Quest of India series*, edited by E. C. DEWICK and MURRAY T. TITUS). New York, Oxford University Press, 1930. ISIS

Reviewed by FRANKLIN EDGERTON, *American Historical Review*, 36, 443-44, 1931; by L. BEVAN JONES, *The Moslem World*, 20, 414-17, 1930.

Tkatsch, Jaroslaus. Die arabische Übersetzung der Poetik des ARISTOTELES und die Grundlage der Kritik des griechischen Textes. I. Band. 283 p. (*Akademie der Wissenschaften in Wien. Philos.-histor. Klasse.* Kommission für die Herausgabe der arabischen ARISTOTELES-Übersetzungen I). Wien, HÖLDER-PICHLER-TEMPSKY, 1928. ISIS

Reviewed by MARTIN PLESSNER, *OLZ*, 34, 1-14, 1931.

Würschmidt, Joseph. Die Schriften GEDOSIS über die Höhenparallelen und über die Sinustafel. (Zum Gebrauch des Quadranten im Islam.) *Sitzungsberichte der Physikalisch-medizinischen Sozietät zu Erlangen.* 60, 127-54, 1928. ISIS

Analysis of a Turkish treatise on the quadrant by SULAIMAN MURAD IBN 'UMAR IBN AHMAD IBN SA'ADI AL-GEDOSI, 1851/52. G. S.

PART III

SYSTEMATIC CLASSIFICATION

Including only the material which could not be included in Parts I and II. Hence studies on Japanese astronomy or on XIIIth century astronomy are not classified below under astronomy, but above, respectively under Japan (in Part II) and S. XIII (in Part I).

The sections forming Part III are classified as follows :

I. *Science in general* : 15. Bibliography; 16. History; 17. Organization; 18. Philosophy.

II. *Formal sciences* (Knowledge of forms) : 19. Logic and Theory of Knowledge; 20. Mathematics; 21. Statistical methods.

III. *Physical sciences* (Knowledge of inorganic nature) : 22. Mechanics; 23. Astronomy; 24. Physics; 25. Chemistry (Physico-chemistry, Industrial chemistry); 26. Technology.

IV. *Biological sciences* (Knowledge of organic nature) : 27. Biology (Generalities, « Natural history »); 28. Botany (Agronomy, Phytopathology, Palaeobotany); 29. Zoology.

V. *Sciences of the earth* (implying knowledge of both organic and inorganic nature) : 30. Geodesy; 31. Geography and Oceanography; 32. Geology, Mineralogy, Palaeontology, Mining; 33. Meteorology, Climatology and Terrestrial Physics.

VI. *Anthropological and historical sciences* (Knowledge of man, past and present) : 34. Anatomy; 35. Physical anthropology (anthropometry and races of man); 36. Physiology (human and comparative); 37. Psychology (human and comparative); 38. Archaeology (generalities, methods); 39. Prehistory; 40. Ethnology (primitive and popular science); 41. Superstition and Occultism.

42. Economics (economic doctrines and history; commerce; transportation and communications); 43. Sociology, Jurisprudence and Positive polity ; 44. History of civilization (general history, historical methods, biography and chronology); 45. History of art (Art and science ; Iconography; Arts and crafts) ; 46. History of language, writing and literature ; 47. History of morals (Moral organization of society); 48. History of philosophy ; 49. History of religion (Science and religion).

VII. *Medicine* : 50. History, organization and philosophy of medicine; 51. Epidemiology, History of special diseases. Public health and Social medicine; 52. History of hospitals and of medical teaching; 53. Pharmacy; Pharmacology; Toxicology.

VIII. *Education* (the methods of accumulating, imparting and diffusing knowledge); 54. Education (Generalities, Methods, Colleges, Universities); 55. Academies, Societies, Congresses, National and international

organization of science; 56. Bibliography (Methods, Libraries); 57. Museology (Museums and Collections).

IX. 58. Alia; 59. Errata.

The applied sciences are not disconnected from the pure sciences, but, as far as possible, the principles and applications of science are considered in the same section. However, applications based upon the principles of many sciences are dealt with separately (e. g. technology; medicine; education).

I. SCIENCE IN GENERAL

16. — HISTORY OF SCIENCE

Castiglioni, Arturo. Il rinascimento scientifico italiano nella storia della civiltà europea. Relazione al Congresso Int. di Storia della Medicina, Roma, 1930. *Minerva Medica*, Anno XXI, n. 47, 20 p., 1930. ISIS

Comité international et Centre international d'histoire des sciences. (Secrétariat et Centre de travail : Hôtel de Nevers, 12 rue Colbert, Paris 2e). *Isis*, 14, 420, 1930. ISIS

Dampier-Whetham, William Cecil. A history of science and its relations with philosophy and religion. XXI+514 p., 14 fig. Cambridge, University Press, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 263-65, 1930.

Diergart, Paul. *Proteus*. Band 1. Verhandlungsberichte der Rheinischen Gesellschaft für Geschichte der Naturwissenschaft, Medizin und Technik, mit Festgabe für WILHELM HABERLING zum sechzigsten Geburtstag. 281 p. Bonn, NEUENDORFF, 1931. ISIS

This first volume of *Proteus* is divided into two parts. The first contains a series of papers written in Dr. WILHELM HABERLING's honor by PAUL DIERGART, ADOLF DYROFF, ALDO MIELI, JAN GERARD DE LINT, KARL SUDHOFF, EDMUND O. VON LIPPMANN, KARL SCHMIZ, GEORG STICKER. These papers are included in the present bibliography each in its proper section. We beg Dr. HABERLING to accept our congratulations and our best wishes for many more years to come.

The second part (p. 47-281) concerns the activities of the Rheinische Gesellschaft during the years 1920-30. It includes summaries of some 210 lectures delivered in many cities of the Rhine region, chiefly in Bonn, with excellent indices.

This new series is called *Proteus* in remembrance of another *Proteus*, « Zeitschrift für Geschichte der gesamten Naturlehre », edited by K. W. G. KASTNER in Erlangen, 1828. The first *Proteus* did not survive its first volume; let us hope that the second one will be more fortunate. G. S.

Evans, Lewis (1853-1930). Obituary by R. T. GUNTHER, *Nature*, 126, 692-93, 1930. ISIS

(History of Science Society). Council meeting of the History of Science Society. Joint meeting of the History of Science Society with the American Historical Association (Durham and Chapel Hill, North Carolina, January 1, 1930). *Isis*, 14, 6-7, 1930. ISIS

(History of Science Society). Fifth list of members of the History of Science Society. Members elected from January 1, 1929 to July 1, 1930. *Isis*, 14, 280-84, 1930. ISIS

(History of Science Society). Section L of the American Association for the Advancement of Science. *Isis*, 14, 220-21, 1930. ISIS

Hoover, C. F. The significance of the scientific conquest of the air for intellectual freedom. *Annals of Medical History*, 2, 651-59, 1930.

(International Committee of the History of Science). List of full and corresponding members at the end of 1929. *Isis*, 14, 219-220, 1930. ISIS

Lenard, Philipp. Grosse Naturforscher. Eine Geschichte der Naturforschung in Lebensbeschreibungen. Zweite verm. Aufl. 332 p., 70 portraits. München, LEHMANN, 1930. ISIS

Reviewed by H. WIELEITNER, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 7, 1931.

Mieli, Aldo. La storia della scienza come disciplina scientifica. *Proteus*, 1, 13-16, 1931. ISIS

Sarton, George. Introduction to the History of Science, vol. 1. From HOMER to OMAR KHAYYAM. XII+840 p. Washington, D.C., Carnegie Institution; London, BALLIERE, TINDALL and COX, 1929. ISIS

Reviewed by MARTIN SPRENGLING, *American Journal of Semitic Languages and Literatures*, 47, 59-60, 1930.

Sarton, George. Medallic illustrations of the history of science. (First series: XIXth and XXth centuries). Eighth article. *Isis*, 14, 215-18, 9 fig., 1930. ISIS

Medals no. 41 to 47: C. A. WURTZ: JOSEPH LEIDY; AUGUSTE COMTE; J. B. LAMARCK; M. F. X. BICHAT; J. V. PONCELET; D. J. F. ARAGO.

Sarton, George. Medallic illustrations of the history of science. (First series : XIXth and XXth centuries). Ninth article. *Isis*, 14, 417-19, 4 pl., 1930. ISIS

Medals no. 48. F. E. MAUPAS; no. 49. A. T. BRONGNIART; no. 50. H. MOISSAN; no. 51. E. G. SAINT-HILAIRE; no. 52. L. F. J. BERTRAND; no. 53. J. H. FABRE.

Sarton, George. Twenty-eighth Critical bibliography of the history and philosophy of science and of the history of civilization (to January 1930). *Isis*, 14, 459-578, 1930. ISIS

« This Twenty-eighth Bibliography contains about 750 items, some of which have been kindly contributed by the following scholars :

E. W. ADAMS (Hertford), H. G. BÉVENOT (Weingarten, Württemberg), M. BLOCH (Leningrad), L. GUINET (Brussels), E. HONIGMANN (Breslau), A. POGO (Cambrigde, Mass.), V. V. RAMANA-SASTRIN (Vedaraniam, S. India), L. THORNDIKE (New York), H. R. VIETS (Boston). »

Schimank, Hans. Epochen der Naturforschung. LEONARDO, KEPLER, FARADAY. 320 p. Berlin, Volksverband der Bücherfreunde, 1930. ISIS

Reviewed by H. WIELEITNER, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 8, 1931.

Tannery, Paul. (1843-1904) Mémoires scientifiques. Publiés par J. L. HEIBERG et H. G. ZEUTHEN. IX. Philologie, 1880-1928. Edité par J. L. HEIBERG. XXXIV+398 p., 5 pl. Toulouse, PRIVAT, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 426-30. 1930.

17. — ORGANIZATION OF SCIENCE

(Internal organization is meant, see Isis, 1, 195. *For external organization, national or international, see section* 55.*)*

Richardson, Hugh. Science and international cooperation. Wheelbirks, Stocksfield, Northumberland, October, 6, 1929. *Isis*, 14, 222-25, 1930. ISIS

Saidla, Leo E. ; Gibbs, Warren E. Science and the scientific mind. New York, MCGRAW-HILL, 1931. ISIS

Twenty-four essays by leading scientists, each dealing with the scientific method in its relation to the various phases of culture.

18. — PHILOSOPHY OF SCIENCE

Barry, Frederick. The scientific habit of thought. An informal discussion of the source and character of dependable knowledge. XIII+358 p. New York, Columbia University Press, 1927. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 265-68, 1930.

Jeans, Sir James. The mysterious universe. IX+154 p., 2 pl. Cambridge, University Press, 1930. ISIS

Reviewed by HERBERT DINGLE, *Nature*, 126, 799-800, 1930.

Millikan, Robert A. Science and the new civilization. 194 p. New York, SCRIBNER'S, 1930. ISIS

Reviewed by GEORGE SARTON, *Isis*, 446-49, 1930.

II. FORMAL SCIENCES

20. — MATHEMATICS

Davis, Harold Thayer. The theory of the VOLTERRA integral equation of second kind. Study nos. 88, 89, 90. 76 p. (*Indiana University studies*, vol. 17. Contribution no. 52, Waterman Institute for scientific research, the Indiana University; presented to the Indiana section of the Mathematical Association of America, May 4, 1929.) 1930. Indiana University Book Store, Bloomington, Indiana. Paper cover $ 1, cloth $ 1.25. ISIS

That type of equation was introduced by S. D. POISSON in his theory of induced magnetism in 1823, but it is rightly called after VOLTERRA because of the latter's fundamental investigations on the subject. DAVIS has devoted himself for many years to the study of integral equations. Beautiful portrait of VOLTERRA in frontispiece. G. S.

Kaufmann, Felix. Das Unendliche in der Mathematik und seine Ausschaltung. X+203 p. Leipzig, DEUTICKE, 1930. ISIS

Reviewed by ORRIN FRINK, JR. in *Bull. Amer. Math. Soc.*, 37, 149-50, 1931.

Klein, Felix (1849-1925). Famous problems of elementary geometry. The duplication of the cube, the trisection of an angle, the quadrature of the circle. An authorized translation of F. KLEIN'S *Vorträge über ausgewählte Fragen der Elementargeometrie*; ausgearbeitet von F. TÄGERT. By WOOSTER WOODRUFF BEMAN (1850-1922) and DAVID EUGENE SMITH. Second edition revised, and enlarged with notes by RAYMOND CLARE ARCHIBALD. XI+92 p. New York, STECHERT, 1930. ISIS

This new edition of a mathematical classic is very welcome. KLEIN's Vorträge published in 1895 are too well known to every mathematician —either in the original German, or in French, Italian, English and Russian translations—to need any recommendation. The new English edition prepared by Prof. ARCHIBALD of Brown University contains additional notes on Gaussian polygons, the construction of the regular heptadecagon, constructions with ruler and compasses, the irrationality of π, the measurement of the circle, EULER's formula $e^{ix} = \cos x + i \sin x$, etc. It would have been appropriate to add also a brief biography of the author. G. S.

Kowalewski, Gerhard. Alte und neue mathematische Spiele : eine Einführung in d. Unterhaltungsmathematik. VI+145 p. Leipzig, TEUBNER, 1930. ISIS

Krull, Wolfgang. Über die ästhetische Betrachtungsweise in der Mathematik. *Sitzungsberichte der Physikalisch-medizinischen Sozietät zu Erlangen*, 61, 207-20, 1929. ISIS

Lietzmann, Walther. Lustiges und Merkwürdiges von Zahlen und Formen. Allerlei Unterhaltungsmathematik, von den Zahlen, von den geometrischen Formen. Vierte durchgesehene und ergänzte Auflage. VI+307 p., 20 pl. Breslau, HIRT, 1930. ISIS

Lietzmann, Walther. Mathematik und bildende Kunst. 150 p. 131 fig. Breslau, HIRT, 1930. ISIS

Miller, George Alfred. Harmony as a principle of mathematical development. *Proc. Nat. Acad. Sc. USA.*, 14, 214-17, 1928. ISIS

Nicod, Jean. Foundations of geometry and induction. With prefaces by BERTRAND RUSSELL and ANDRÉ LALANDE. 284 p. New York, HARCOURT, BRACE and Co., 1930. ISIS

Reviewed by ARNOLD DRESDEN in *Bull. Amer. Math. Soc.*, 37, 152-53, 1931.

Rainoff, T. ALEXANDRE VASSILIEVIČ VASSILIEFF. (Né le 6 août 1855. Décédé le 6 octobre 1929). *Isis*, 14, 342-48, 1930. ISIS

Biography with portrait.

Rome, A. Histoire des mathématiques. *Revue des questions scientifiques*, 18 p., 5 fig., 1930. ISIS

Comptes rendus de publications récentes sous les titres suivants : le comput pascal de MICHEL PSELLUS; PAUL TANNERY philologue; Sur l'invention de la preuve par 9; Sur l'astrolabe plan; Mathematici graeci minores atque minimi; Survivances de la méthode d'ARCHIMÈDE.

Rome, A. Procédés anciens de calcul des combinaisons. *Annales de la Société scientifique de Bruxelles*, 40, 97-104, 1 fig., 1930. ISIS

1. Une scolie aux data d'EUCLIDE; 2. Formules de combinaisons chez BHASCARA; 3. Formules de combinaisons chez BOÈCE.

Simons, Lao G. FLORIAN CAJORI. *American Mathematical Monthly*, 37, 460-62, 1930. ISIS

Smith, David Eugene. Catalogue of mathematical instruments. *The Industrial Museum of New York*, 1, 58, 1930. ISIS

Smith, David Eugene. A source book in mathematics. XVII+701 p., 9 pl., figs. New York, MCGRAW-HILL, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 268-70, 1930.

Vetter, Q. Jak se počitalo a měřilo na úsvitě kultury. By Q. VETTER. « Melantrich » v Praze. Lidová Universita. Svazek XV, 141 p., (Wie gerechnet und gemessen wurde zur Zeit des kulturellen Erwachens. Von Professor QUIDO VETTER, Prague, 1926). ISIS

Reviewed by RUTH STRUIK, *Isis*, 14, 449-50, 1930.

21. — STATISTICS

(History and methods. Tables and generalities. For the applications, refer to the sciences to which they are applied)

March, Lucien. Les principes de la méthode statistique. Avec quelques applications aux sciences naturelles et à la science des affaires. 808+XII p., 50 fig. Paris, ALCAN, 1930. ISIS

III. PHYSICAL SCIENCES

(Knowledge of inorganic nature)

22. — MECHANICS

(Including celestial and atomical mechanics)

Gunn, John Alexander. The problem of time : an historical and critical study. 460 p. London, ALLEN and UNWIN, 1929. ISIS

Reviewed by THOMAS GREENWOOD, *Nature*, 126, 677-79, 1930.

23. — ASTRONOMY

Antoniadi, E. M. Le planète Mars : étude basée sur les résultats

obtenus avec la grande lunette de l'Observatoire de Meudon et exposé analytique de l'ensemble des travaux exécutés sur cet astre depuis 1659. IX+240 p., 10 pl. Paris, HERMANN, 1930. ISIS

Reviewed in *Nature*, 127, 51-53, 1931; by A. V., *Revue générale des sciences*, 41, 584, 1930.

Bigourdan, G. Le bureau des longitudes. Annuaire pour l'an 1931. Notice A, 9 fig., 1931. ISIS

Bork, Ferdinand. Vorstufen unseres Kalenders in der Volkskunde und im Kalender. *Das Weltall*, 30, 9-11, 1930. ISIS

Court, Thomas H., Rohr, Moritz von. A history of the development of the telescope from about 1675 to 1830 based on documents in the Court collection. *Trans. Opt. Soc.* 30, 207-60, 27 fig., 1928-29. ISIS

Dyson, Sir Frank W. Histoire de l'observatoire de Greenwich. *Revue scientifique*, 69, 1-13, 27 fig., 1931. ISIS

Filippoff, L. Sur la détermination de l'époque de la disparition de l'Atlantide. *Comptes Rendus*, 191, 393, 1930. ISIS

L'extrait suivant de la note de M. L. FILIPPOFF, présentée par M. G. BIGOURDAN à l'Académie des Sciences est classé dans les Comptes Rendus sous « Astronomie ». C'est plutôt de l'atlantidomanie.

« M'occupant depuis quelque temps du problème atlantéen, j'ai essayé de déterminer l'époque de la disparition de l'Atlantide par une méthode astronomique très simple. J'ai trouvé que la tradition sur l'Atlantide s'est conservée non seulement dans l'ancienne Egypte, comme le disait PLATON, mais aussi dans les légendes diluviennes de l'Amérique préhistorique. Ces deux traditions, d'origines si différentes, s'accordent cependant sur l'époque de la disparition de l'Atlantide. Elles la placent à l'époque où le point vernal se trouvait dans le signe zodiacal de l'Écrevisse (tradition égyptienne) et plus précisément au moment où il passait près de l'étoile ϵ de cette constellation (tradition mexicaine). J'ai cherché, par conséquent, à déterminer si la trajectoire du point vernal passait près de cette étoile et à quelle époque cette coïncidence avait eu lieu. Le calcul montre très aisément que le point vernal se trouvait, réellement, tout près de Praesepe Cancri l'an 7256 avant J. C. L'Atlantide existait donc encore pendant l'époque quaternaire. Elle n'a disparu que vers l'an 7256 avant notre ère, ce qui confirme bien la date indiquée par PLATON ».

Malheureusement, les Égyptiens de l'an 7256 ne connaissaient pas le signe zodiacal de l'Écrevisse. Dans un article intitulé « Les précurseurs d'HIPPARQUE » (*Isis*, 16, 514-16..), M. L. FILIPOPFF commet la même erreur fondamentale : il admet implicitement que les Égyptiens connaissaient nos signes zodiacaux et que leurs limites des constellations coïncidaient avec celles indiquées par ARGELANDER dans son « Uranometria Nova ». Il est inutile

d'ajouter que les « Mexicains » de l'an 7256 ne savaient pas que la précession existe, que le soleil se trouvait, lors de l'équinoxe de printemps, près de l'étoile que nous appelons ε Cancri, et que le moyen le plus scientifique de transmettre à la postérité la date de la « dispariton de l'Atlantide » consistait dans l'indication de la position du point vernal à l'époque de la catastrophe.

Une remarque purement astronomique : si le point vernal se trouvait « tout près de Praesepe Cancri l'an 7256 avant J. C. », il s'y trouvait, sans doute, l'an 7250 et même l'an 7200 ou 7300; cette date n'est, d'ailleurs, connue qu'à un multiple de 26000 ans près... A. P.

Ginsburg, Jekuthiel. The astrolabe. *The Industrial Museum of New York*, 1, 1-14, 5 fig., 1930. ISIS

Klüber, Harald von. Die Sternkunde bei den Völkern der Sundainseln. *Die Sterne*, 11, 30-50, 3 figs., 2 pl., 1931. ISIS

Neugebauer, Paul Victor. Astronomische Chronologie. Vol. 1, text, XII+190 p., 17 figs. Vol. 2, tables, 136 p. Berlin, DE GRUYTER, 1929. ISIS

Reviewed by A. POGO, *Isis*, 14, 450-54, 1930.

Olivier, Charles P. Comets. 246 p., ill. Baltimore, WILLIAMS & WILKINS, 1930. ISIS

« It seemed that a book of moderate size, which would cover briefly the history of the subject and the present theories of origin, constitution, changes, and dissolution of comets, would be useful to the astronomer who does not specialize in the subject, as well as to the average intelligent reader. It is the author's aim to fill these requirements, leaving out entirely any mathematical discussions. » The historical section (p. 1-15) is insignificant.

Wolf, Rudolf (1816-93). Biography by E. DOUBLET. *Revue scientifique*, 658-60, 1930. ISIS

Yalden, J. ERNEST G. YALDEN. *The Industrial Museum of New York*, 19-56, 6 pl., 5 fig., 1930. ISIS

24. — PHYSICS

Katalog einer Bilderausstellung zur Geschichte der Brille. Im Auftr. d. Vorst. d. XIII. Internat. ophthalm. Kongresses z. Amsterdam zusammengest. v. R. GREEFF, O. HALLAUER, K. LUNDSGAARD, A. v. PFLUGK, W. REISS, SIMON, H. J. M. WEVE. 266 p., fig. Amstasdam, D'OLIVEIRA, 1929. ISIS

Rohr, Moritz v. Die optischen Instrumente. Brille, Lupe, Mikroskop,

Fernrohr, Aufnahmelinse und ihnen verwandte Vorkehrungen. 4., verm. u. verb. Aufl. VI+130 p., 91 fig., Berlin, SPRINGER, 1930. ISIS

Schimank, H. Geschichte des Energieprinzips. *Beiträge zur Geschichte der Technik und Industrie*, 20, 31-46, 5 fig., 1930. ISIS

25. — CHEMISTRY, PHYSICO-CHEMISTRY, INDUSTRIAL CHEMISTRY

Abel, John J. Chemistry in relation to biology and medicine with especial reference to insulin and other hormones. *Science*, 56, 307-19, 337-46, 1927. ISIS

Abel, John J. Chemistry in relation to medicine. *Journal of Chemical Education*, 6, 1045-64, 1929. ISIS

Bugge, Günther. Das Buch der grossen Chemiker. Band I : von ZOSIMOS bis SCHÖNBEIN. XII+496 p., 62 fig. Berlin, Verlag Chemie, 1929. ISIS

Includes biographies of the following chemists by various authors : ZOSIMOS, DSCHÂBIR, ALBERTUS MAGNUS, ROGER BACON, RAYMUNDUS LULLUS, PSEUDO-GEBER, BIRINGUCCIO, PARACELSUS, AGRICOLA, LIBAVIUS, PSEUDO-BASILIUS VALENTINUS, VAN HELMONT, GLAUBER, BOYLE, STAHL, BOERHAAVE, GEOFFROY L'AÎNÉ, MARGGRAF, BLACK, CAVENDISH, PRIESTLEY, SCHEELE, LEBLANC, LAVOISIER, KLAPROTH, BERTHOLLET, PROUST, FOURCROY, VAUQUELIN, RICHTER, DALTON, GAY-LUSSAC, THENARD, DAVY, FARADAY, BERZELIUS, MITSCHERLICH, SCHÖNBEIN.

Bugge, Günther. Das Buch der grossen Chemiker. Unter Mitwirkung namhafter Gelehrter. Band II : von LIEBIG bis ARRHENIUS. X+559 p., 78 fig. Berlin, Verlag Chemie, 1930. ISIS

Contains biographies of the following chemists by various authors : JUSTUS LIEBIG, FRIEDRICH WÖHLER, DUMAS, GRAHAM, BUNSEN, GERHARDT, LAURENT, WURTZ, KOLBE, A. W. v. HOFMANN, PASTEUR, CANNIZZARO, BERTHELOT, KEKULÉ, PETER GRIESS, LOTHAR MEYER, MENDELEJEFF, RAMSAY, CROOKES, HEINRICH CARO, ADOLPH FRANK, ADOLF VON BAEYER, CLEMENS WINKLER, GEORG LUNGE, HEINRICH v. BRUNCK, VICTOR MEYER, VAN'T HOFF, EMIL FISCHER, EHRLICH, ARRHENIUS.

Couch, James F. Early history of percolation. *Am. Jour. of Pharmacy*, 91, 16-25, 1919. ISIS

Couch, James F. The evolution of chemical terminology. III. The « Micella ». V. Some alchemical terms. VI. Hydroxide. *American*

Journal of Pharmacy, 469-76, 1922 ; 227-32, 533-54, 1923. ISIS

Millikan, Robert A. Present status of theory and experiment as to atomic disintegration and atomic synthesis. *Nature*, 127, 167-70, 1931. ISIS

Retiring presidential address to the American Association for the Advancement of Science, delivered at Cleveland on Dec. 29, 1930.

Spackman, Charles. Some writers on lime and cement from CATO to present time. XVII+287 p. Cambridge, HEFFER, 1929. ISIS

Reviewed by S. R. HIND, *Nature*, 127, 156-58, 1931.

26. — TECHNOLOGY

(For mining, see 32. *Geology ; for industrial chemistry,* 25. *Chemistry. See also Arts and Crafts under* 45*)*

Becker, Fritz. Die Betriebswirtschaftliche Entwicklung im Baugewerbe. *Beiträge zur Geschichte der Technik und Industrie*, 20, 137-46, 8 fig., 1930. ISIS

Geisler, Kurt W. Aus der Geschichte der Bierbrauerei. *Beiträge zur Geschichte der Technik und Industrie*, 20, 130-36, 7 fig., 1930. ISIS

Lewton, Frederick L. The servant in the house : a brief history of the sewing machine. *Annual Report of the Smithsonian Institution*, 559-83, 8 pl., 1929. ISIS

Matchoss, Conrad. Vom Ingenieur, seinem Werden und seiner Arbeit in Deutschland. *Beiträge zur Geschichte der Technik und Industrie*, 20, 1-20, 11 fig., 1930. ISIS

Meller, Emil. Aus der Geschichte des Getreidemühlenbaus. *Beiträge zur Geschichte der Technik und Industrie*, 20, 111-18, 9 fig., 1930. ISIS

Mercer, Henry C. Obituary by F. M. FELDHAUS, *Isis*, 14, 424, 1930. ISIS

Mitman, Carl W. The beginning of the mechanical transport era in America. *Annual Report of the Smithsonian Institution*, 507-58, 24 pl., 8 fig., 1929. ISIS

Moll, Friedrich. Das Schiff in der bildenden Kunst vom Altertum

bis zum Ausgang des Mittelalters. 85 p., 102 pl., Bonn, KURT SCHROEDER, 1929. ISIS

Reviewed by F. M. FELDHAUS, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 30, 25, 1931; by AUG. KÖSTER, *OLZ*, 33, 865-66, 1930.

Moll, Friedrich. Geschichte der Holzschutzindustrie in Deutschland. *Beiträge zur Geschichte der Technik und Industrie*, 20, 26-30, 7 fig., 1930. ISIS

Pfeiffer, Otto. Zur Vorgeschichte der Schreibmaschine. Die Modelle des Technischen Museums in Wien. *Beiträge zur Geschichte der Technik und Industrie*, 20, 149-68, 5 fig., 1930. ISIS

Pflugk, Albert v. Die Ordnungen der Nürnberger Brillenmacher. *Forsch. z. Gesch. d. Opt.* 1, 203-222, 1 fig., 1930. ISIS

Rohr, Moritz, v. Zur Geschichte der Zeissischen Werkstätte bis zum Tode ERNST ABBES. Mit Beiträgen von MAX FISCHER und AUGUST KÖHLER. *Forschgn. Gesch. Optik.* (Beil.hefte z. Instr.kde), 1, 91-202, 10 S. Anlagen, 49 fig., 1930. ISIS

Schröder, Albert. Die Wasserkunst und das Brunnennetz in Alt-Leipzig. *Beiträge zur Geschichte der Technik und Industrie*, 20, 119-25, 17 fig., 1930. ISIS

Schwenninger, Oskar. Die Entwicklung der Schwarzwälder Uhrenindustrie. *Beiträge zur Geschichte der Technik und Industrie*, 20, 91-98, 11 fig., 1930. ISIS

IV. BIOLOGICAL SCIENCES
(Knowledge of organic nature)

27. BIOLOGY

(Generalities, « Natural History »)

Boycott, A. E. The transition from live to dead: the nature of filtrable viruses. *Annual Report of the Smithsonian Institution*, 323-43, 1929. ISIS

Reprinted from the *Proceedings of the Royal Society of Medicine*, November 1928.

Caullery, Maurice. Génétique et évolution. *Revue générale des sciences*, 41, 567-73, 1930. ISIS

Donnan, F. G. The mystery of life. *Annual Report of the Smithsonian Institution*, 309-21, 1929. ISIS

Evening discourse before the British Association for the Advancement of Science at the Glasgow Meeting, 1928. Reprinted by permission of the association.

Guyénot, Emile. La variation et l'évolution. 2 vol. de l'Encyclopédie scientifique. t. 1 : La variation, 457 p., 46 fig. t. 2 : L'évolution, 414 p., 5 fig., Paris, DOIN, 1930. ISIS

Reviewed by L. CUÉNOT, *Revue générale des sciences*, 41, 587-88, 1930.

28. — BOTANY

(Agronomy, phytopathology, palaeobotany)

Hirschfeld, Ernst. Studien zur Geschichte der Heilpflanzen. *Kyklos*, 2, 145-79, 5 fig., 1929. ISIS

1. Lilium convallium; 2. Scilla.

Tamiya, H. u. Morita, S. Bibliographie von *Aspergillus* 1729-1928. *Bot. Mag. Tokyo*, 43, 60-71, 145-56, 179-89, 237-49, 281-91, 321-32, 371-81, 427-38, 501-15, 1929. ISIS

Tscholakowa, Maria. Zur Geschichte der medizinischen Verwendung des Safran *(Crocus sativus)*. *Kyklos*, 2, 179-90, 1929. ISIS

29. — ZOOLOGY

Abel, John J.; Macht, David I. The poisons of the tropical toad, *Bufo Agua*. A preliminary communication. *Journal of the American Medical Association*, 56, 1531-1536, 1911. ISIS

Bodenheimer, F. S. Materialien zur Geschichte der Entomologie bis LINNÉ. Band II. VI+496 p., 100 Abb. Berlin, JUNK, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 454-56, 1930.

Dean, Bashford (1867-1928). The BASHFORD DEAN memorial volume. Archaic fishes. Edited by EUGENE WILLIS GUDGER. Article I. Memorial of BASHFORD DEAN, by WILLIAM K. GREGORY. 42 p., 8 pl. New York, American Museum of Natural History, 1930. ISIS

Elaborate biographical sketch of the great ichthyologist and student of arms and armor, admirably published with many portraits and a long bibli-

ography (315 items, 1887 to 1929). DEAN's Bibliography of fishes (3 vols., New York, 1916-23) was discussed in our vol. 6, 456-59. The following extract illustrates the unity of his mind: « A growing knowledge of the chronological sequence of types finally results in a general concept of the evolution of arms and armor as it was even after the invention of gunpowder drove them into new and strange reincarnations and vestigial remnants. Here DEAN's authentic knowledge of the ways of organic evolution gave him an immense advantage over the mere historian. For example, his drawings of the evolution of various lines of helmets and pole arms in Europe strongly recall the phylogenetic tree of certain groups of fossil cephalopods, for in both cases a certain very primitive and ancient pattern gives rise to gradually diverging series which toward the end take on the 'phylogerontic' extremes that precede sudden extinction, through failure to meet new changes in the environment. Some zoologists have objected to the stretching of the word 'evolution' to cover such an historical process, but, as DEAN showed, the evolution of arms and armor is only a sort of biological evolution once removed, the conservatism of armorers and purchasers taking the place of heredity in tending toward continuity, and the stress of environment evoking mutational changes in type. » (p. 17). G. S.

(Eckstein, Karl). Festschrift zum 70. Geburtstage von KARL ECKSTEIN. Mit Beiträgen von FR. BEYER. K. BUGOW, E. CONTAG... 155 p., port., fig. Berlin, Verlag d. Fisch.-Ver. f. d. Prov. Brandenburg e. V., 1929. ISIS

Efflatoun, Bey Hassan C. The development of entomological science in Egypt. *Trans. 4th Int. Congr. Entomol.*, 2, 737-42, 1929. ISIS

Groos, Karl. Die Spiele der Tiere. Dritte Auflage. VI+222 p. Jena, FISCHER, 1930. ISIS

Howard, Leland Ossian. A history of applied entomology. 51 pl. Washington, D.C., Smithsonian Institution. ISIS

Kraemer, H. Die Stellung der Tierzucht zur Abstammungslehre. *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik*, 13, 113-32, 1930. ISIS

Mohr, Erna. Die Elefanten-Darstellung in der Kathedrale zu Exeter. *Zoolog. Garten*, 3, 213-15, 1 fig., 1930. ISIS

Mostra della illustrazione zoologica in opere a stampa dal sec. XVI al XVIII. Catalogo. XI Congresso Internazionale di Zoologia, Padova, 4-11 settembre, 1930. 46 p., 11 pl. ISIS

Reinke, Johannes. Wissen und Glauben in der Naturwissenschaft

mit besonderer Berücksichtigung der Tierpsychologie. v+112 p., map. Leipzig, BARTH, 1929. ISIS

V. SCIENCES OF THE EARTH

(Implying knowledge of both organic and inorganic nature)

30. — GEODESY

Cajori, Florian. A century of American geodesy. *Isis*, 14, 411-16, 7 fig., 1930. ISIS

31. — GEOGRAPHY AND OCEANOGRAPHY

Brandt, Bernhard. Mittelalterliche Weltkarten aus Toscana. 8 p., 5 pl. Prag., Staatsdruckerei, 1929. ISIS

Colin, Elicio. XXXIX[e] Bibliographie géographique 1929. Publiée avec la collaboration de l'American Geographical Society, du Comitato Geografico Nazionale Italiano, de la Royal Geographical Society (London), de la Société Royale de Géographie d'Egypte, et avec le concours de la Fédération des Sociétés françaises de sciences naturelles. 686 p. Paris, COLIN, 1930. ISIS

This excellent bibliography is well known to our readers, for this is the seventh of its yearly volumes which we bring to their attention. For the preceding one see *Isis*, 15, 304. The present volume contains 3045 items (some of which quite long), duly classified and indexed. The sections on history of geography and historical geography contain respectively 121 and 126 notes. G. S.

Halldór, Hermannsson. Two cartographers. GUDBRANDUR THORLÁKSSON (1542-1627) and THÓRDUR THORLÁKSSON (1637-1697). *Islandica*, 17, 44 p., 7 maps, 1926. ISIS

Helbronner, Paul. Histoire sommaire de la représentation cartographique de la Corse. *Revue générale des sciences*, 41, 509-18, 1930. ISIS

Honigmann, Ernst. Die sieben Klimata und die *πόλεις ἐπίσημοι*. Eine Untersuchung zur Geschichte der Geographie und Astrologie im Altertum und Mittelalter. 247 p., 4 fig. Heidelberg, WINTER, 1929. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 270-76, 1 fig., 1930. Corrections, ibid., 554.

L'Honoré Naber, S. P. Reisebeschreibungen von Deutschen Beamten

und Kriegsleuten im Dienst der Niederländischen West- und Ost-Indischen Kompagnien. 1602-1797. Haag, NIJHOFF, 1930. ISIS

Marmer, H. A. The Gulf Stream and its problems. *Annual Report of the Smithsonian Institution*, p. 285-307, 5 fig., 1929. ISIS

Reprinted from the *Geographical Review*, July 1929.

Reparaz (fill), Gonçal de. ELS PRUNES, cartògrafs catalans dels segles XVI i XVII (A propòsit d'uns mapes inèdits d'aquests cartògrafs de Mallorca). 15 p., 2 pl. Tiratge a part dels *Estudis Universitaris Catalans*, vol. XIII. Barcelona, 1929 (In Catalan). ISIS

Reparaz, Gonzalo de. Geografía y política. Veinticinco lecciones de historia naturalista. Colección GONZALO DE REPARAZ, tomo II. Barcelona, MENTORA, 1929. ISIS

GONZALO DE REPARAZ (b. in Oporto, 1860), the geographer and publicist, states his point of view in the first lección : « la politica no es sino un producto geográfico ». Then follow thirteen lecciones devoted to the interaction of geography and politics in Russia, England and Spain. Of the remaining eleven lecciones the following are of interest to the historian of science : XV. El vuelo del « Plus Ultra » y el viaje de COLÓN ; XVI. La vocación marítima de Portugal es consecuencia de la situación geográfica ; XVII. Un imperio español en el Mediterráneo oriental ; XVIII. El camino de la India estaba descubierto antes que COLÓN le quisiera descubrir ; XIX. El engaño de Tordesillas ; XX. Los contactos geográficos y sus leyes.

The other volumes of the Colección GONZALO DE REPARAZ which are thus far available, are : La constitución natural de España y las de papel ; Historias que parecen cuentos ; Demolición y reconstrucción ; Catalanismo, Iberismo y Lusitanismo. A. P.

Reparaz, Gonzalo de, (hijo). La época de los grandes descubrimientos españoles y portugueses. Colección Labor, Biblioteca de iniciación cultural, No. 75, 206 p., 15 figs., 12 pl., 5 maps. Barcelona, Labor, 1931. ISIS

A remarkably well written popular book. Numerous footnotes permit the reader to get more detailed information on points of especial interest to him. The contributions of Italians (in spite of the title), Portuguese, and Spaniards are dealt with objectively. Although the epoch of the great discoveries—up to 1550, according to the preface—is the main subject of the book, the author devotes four score of the two hundred pages to Antiquity and the Middle Ages. The concatenation of ideas and events is clearly sketched ; the intentions and achievements of HENRY THE NAVIGATOR and of COLUMBUS, GAMA, and MAGELLAN are discussed with just the right amount of detail. The index occupies ten two-column pages, and proves that the author has succeeded in giving as much information as many a compiler of a dry text book. The well printed facsimiles in the text and on the plates,

and the excellent colored maps add to the value of this very inexpensive book.
A. P.

32. — GEOLOGY, MINERALOGY, PALAEONTOLOGY, MINING

(For palaeobotany, palaeozoology and palaeoanthropology, see respectively, 28. Botany, 29. Zoology and 39. Prehistory)

Gough, J. W. The mines of Mendip. VIII+269 p. New York. Oxford University Press, 1930. ISIS

Reviewed by ABBOTT PAYSON USHER, *American Historical Review*, 36, 422-23, 1931.

Hodgson, Ernest A. Bibliography of seismology. 100 p. (Dept. of the Interior, Canada, Publ. of the Dominion Observatory Ottawa, vol. 10, no. 1-6). Ottawa, 1929. ISIS

Margerie, Emm. de. La Société géologique de France de 1880 à 1929. 81 p., 20 pl. Extrait du livre jubilaire publié à l'occasion du centenaire de la Société géologique de France, 1830-1930. ISIS

Cette belle étude complète celle publiée par ALBERT DE LAPPARENT en 1880 à l'occasion du cinquantenaire. Ce n'est pas seulement une histoire de la Société géologique, mais aussi de la géologie française, et elle est d'autant plus précieuse que l'auteur pourrait dire comme ENÉE « quorum pars magna fui ». Cette histoire il l'a vécue tout entière. Une admirable collection de portraits illustre ce mémoire. Tous les maîtres de la géologie française sont là devant nos yeux; il ne nous manque qu'un seul portrait, celui que l'auteur seul ne pouvait nous donner. G. S.

Merriam, John C. The living past. XI+144 p., 16 pl. New York, SCRIBNER'S, 1930. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 456-58, 1930.

Scheuermann, Ludwig. Die Fugger als Montanindustrielle in Tirol und Kärnten. Ein Beitrag zur Wirtschaftsgeschichte des 16. und 17. Jahrhunderts. 500 p. (*Studien z. Fugger-Gesch.* Hrsg. v. JAKOB STRIEDER. Bd. 8). München, DUNCKER & HUMBLOT, 1929. ISIS

Reviewed by HEINR. SIEVEKING, *Deutsche Literaturzeitung*, 1, 2442-44, 1930.

VI. ANTHROPOLOGICAL AND HISTORICAL SCIENCES

(Knowledge of man, past and present)

36. — PHYSIOLOGY *(human and comparative)*

Abel, John J. Experimental and chemical studies of the blood with

an appeal for more extended chemical training for the biological and medical investigator. 45 p., 5 fig. Mellon Lecture. (Under the auspices of the Society for Biological Research). University of Pittsburgh, 1915. ISIS

The historical introduction deals with bloodletting, the use of leeches, etc.

Abel, John J. Physiological, chemical and clinical studies on pituitary principles. The Harvey Society Lectures, 1923-1924, p. 155-212, Philadelphia, LIPPINCOTT, 1924. ISIS

Abel, John J. Some recent advances in our knowledge of the ductless glands. *Bulletin of the Johns Hopkins Hospital*, 38, 1-32, 1926. ISIS

Castiglioni, Arturo. Il contributo degli italiani alla fisiologia ed alla patologia del cuore. Dalla Nuova antologia. 1° Dicembre 1930. 16 p. Roma, BESTETTI e TUMMINELLI. ISIS

Chittenden, Russell H. The development of physiological chemistry in the United States. 427 p. (*American Chemical Society Monograph Series*, no. 54). New York, Chemical Catalog Co., 1930. ISIS

Irsay, Stephen d'. A physiological synthesis. *Kyklos*, 2, 117-30, 1929. ISIS

37. — PSYCHOLOGY *(human and comparative)*

Binswanger, Ludwig. Wandlungen in der Auffassung und Deutung des Traumes von den Griechen bis zur Gegenwart. 112 S. Berlin, SPRINGER, 1928. ISIS

38. — ARCHAEOLOGY *(Generalities, methods)*

Allcroft, A. Hadrian. The circle and the cross : a study in continuity. In 2 volumes. Vol. 2 : The cross. VII+454 p., 4 pl. London, MACMILLAN, 1930. ISIS

Blanchet, Adrien. Médailles, jetons, méreaux. VIII+610 p., 88 fig., 8 pl. (Manuel de numismatique française. Par A. BLANCHET et A. DIEUDONNÉ. Tome III.) Paris, PICARD, 1930. ISIS

Reviewed by FRIEDRICH FRHR. V. SCHRÖTTER, *Deutsche Literaturzeitung*, 1, 2392-94, 1930.

Brogger, A. W. Kulturgeschichte des norwegischen Altertums. 46 p.

(Instituttet for sammenlignende kulturforskning, Ser. A : Forelesninger VI.) Oslo, ASCHEHOUG, 1926. ISIS

Reviewed by ERNST WAHLE, *Deutsche Literaturzeitung*, 1, 2238-46, 1930

Dean, Bashford. Handbook of arms and armor, European and oriental. Fourth edition. With additions, corrections, and a chapter on the BASHFORD DEAN Memorial Gallery by STEPHEN V. GRANCSAY. XVIII+331 p. New York, Metropolitan Museum of Art, 1930. ISIS

Jerphanion, G. de, (S. J.). La voix des monuments. Notes et études d'archéologie chrétienne. 320 p., 60 fig., 64 pl. Paris, VAN OEST, 1930. ISIS

Nordenskiöld, Erland. L'archéologie du bassin de l'Amazone. VIII+72 p., 57 pl. (*Ars Americana*, 1.) Paris, VAN OEST, 1930. ISIS

39. — PREHISTORY

Burkitt, M. C. Correlation of the archaeological and geological records. *Nature*, 126, 509-10, 1930. ISIS

Including a table of correlation.

Childe, Vere Gordon. The bronze age. XIII+258 p. Cambridge, University Press, 1930. ISIS

Dart, Raymond. Early man in N. Rhodesia. Discoveries at Mumbwa. *The London Times*, Aug. 22, 1930. ISIS

Menghin, Oswald. Weltgeschichte der Steinzeit. 650 p., 1029 fig., 7 maps. Wien, SCHROLL, 1930. ISIS

« Der Verfasser geht von der prähistorischen Archäologie aus, deren ausserordentliche Fortschritte in den letzten Jahrzehnten unser historisches Weltbild überraschend erweitert haben. Die Darstellung erstreckt sich über die gesamte ältere und jüngere Steinzeit. Sie schliesst in Europa etwa mit 2000 vor Christi Geburt, in den überseeischen Ländern werden aber auch spätere steinzeitliche Kulturen behandelt. Auf festem chronologischem Unterbau werden alle bisher bekanntgewordenen steinzeitlichen Kulturkreise analysiert und in ein grosszügiges kulturgenealogisches System eingeordnet. Zum erstenmal wird auch das aussereuropäische Fundmaterial in vollem Umfange in die wissenschaftliche Betrachtung mit einbezogen. Das Ergebnis ist ein festgefügtes Bild der menschlichen Kulturentwicklung auf Grund des archäologischen Materials, das gestattet, Vergleiche mit den modernen Primitivkulturen auf breitester Basis anzustellen. »

Shetrone, Henry Clyde. The mound-builders : a reconstruction of the life of a prehistoric American race, through exploration and interpretation of their earth mounds, their burials and their cultural remains. XX+508 p. New York, APPLETON, 1930. ISIS

Weinert, Hans. Menschen der Vorzeit. Ein Überblick über die altsteinzeitlichen Menschenreste. 140 p., 61 fig. Stuttgart, ENKE, 1930. ISIS

40. — ETHNOLOGY

(Primitive and popular science)

Bendann, E. Death customs : an analytical study of burial rites. XIII+304 p. *(The History of Civilization Series)*. London, KEGAN PAUL, 1930. ISIS

Reviewed in *Nature*, 127, 8, 1931.

Descamps, Paul. État social des peuples sauvages. Préface de PAUL RIVET. 288+23 p. Paris, PAYOT, 1930. ISIS

Reviewed by M. DUFOUR, *Revue générale des sciences*, 41, 655, 1930.

Harrison, H. S. Evolution in material culture. *Nature*, 126, 726-29, 1930. ISIS

Hempler, Franz. Psychologie des Volksglaubens, insbesondere der volkstümlichen Natur- und Heilkunde des Weichsellandes. 112 p. map. *(Einzelschriften der historischen Kommission für Ost- und Westpreussische Landesforschung*, H. 4). Königsberg i. Pr., GRÄFE und UNZER, 1930. ISIS

Reviewed by M. A. VAN ANDEL, *Bijdragen tot de geschiedenis der geneeskunde*, 10, 319-20, 1930.

Ludendorff, H. Über die Reduktion der Maya-Datierungen auf unsere Zeitrechnung. *Stzsber. Preuss. Ak. Wiss. (phys.-math. Kl.)*, XVIII. Abh., 16 p., 1930. ISIS

Luquet, G. H. L'art primitif. III+267 p. *(Encyclopédie scientifique* : Bibliothèque d'anthropologie.) Paris, GASTON DOIN, 1930. ISIS

Reviewed in *Nature*, 127, 53, 1931.

Nordenskiöld, Erland. Comparative ethnographical studies. Vol. 7, part 2. : Picture-writings and other documents. By NÉLE, CHARLES SLATER, CHARLIE NELSON and other Cuna Indians. IV+75 p., 8 pl. London, University Press, 1930. ISIS

Obermaier, Hugo; Kühn, Herbert. Buschmannkunst: Felsmalereien aus Südwestafrika. Nach d. Aufnahmen von REINHARD MAACK bearbeitet. XI+64 p., 39 pl. Berlin, BRANDUS, 1930. ISIS

Pope, Saxton Temple. A study of bows and arrows. IV+102 p., 20 pl. Berkeley, University of California Press, 1930. ISIS

Stow, George William. Rock paintings in South Africa: from parts of the Eastern Province and Orange Free State. With an introduction and descriptive notes by DOROTHEA F. BLEEK. XXVIII+70 p., 72 pl. London, METHUEN, 1930. ISIS

41. — SUPERSTITION AND OCCULTISM

Gorsleben, Rudolf John. Hoch-Zeit der Menschheit. 730 p., 200 fig. Leipzig, KOEHLER & AMELANG, 1930. ISIS

Guazzo, Brother Francesco Maria. Compendium maleficarum. Collected in three books from many sources by Brother FRANCESCO MARIA GUAZZO, of the Order of S. Ambrose ad Nemus. Edited with notes by MONTAGUE SUMMERS. Translated by E. A. ASHWIN. London, RODKER, 1929. ISIS

Knowlson, Thomas Sharper. The origins of popular superstitions and customs. X+242 p. London, LAURIE, 1930. ISIS

Meyere, Victor de. De tooverij in Vlaanderen. Tooveraars, tooverheksen, aflezers en stuiters. Hunne practijken en geheimen. *Nederlandsch tijdschrift voor volkskunde*, Afl. 4, 5, 6, 1929. ISIS

Reviewed by M. A. VAN ANDEL, *Bijdragen tot de geschiedenis der geneeskunde*, 10, 297, 1930.

Scot, Reginald (1538?-1599). The discoverie of witchcraft. With an introduction by MONTAGUE SUMMERS. XXXVII+283 p. London, RODKER, 1930. ISIS

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Thompson, Charles John Semel. The mystery and lore of monsters With accounts of some giants, dwarfs and prodigies. With a foreword

by Sir D'ARCY POWER. 256 p. London, WILLIAMS and NORGATE, 1930. ISIS

43. — SOCIOLOGY, JURISPRUDENCE AND POSITIVE POLITY

Morris, Richard B. Studies in the history of American law. With special reference to the seventeenth and eighteenth centuries. New York, Columbia University Press, 1930. ISIS

Sorokin, Pitirim A. A survey of the cyclical conceptions of social and historical process. *Social Forces*, 6, 28-40, 1927. ISIS

44. — HISTORY OF CIVILIZATION

(General history, historical methods, biography and chronology)

(Carnegie Institution). List of doctoral dissertations in history now in progress at the chief American universities, December, 1930. 50 p. Department of Historical Research, Carnegie Institution of Washington, 1930. ISIS

Foerster, Norman *(editor)*. Humanism and America. Essays on the outlook of modern civilisation. XVII+294 p. New York, FARRAR and RINEHART, 1930. ISIS

Reviewed by GEORGE SARTON, *Isis*, 14, 446-49, 1930.

Smith, G. Elliot. Human history. 509 p. London, CAPE, 1930. ISIS

Reviewed by J. L. MYRES, *Nature*, 127, 47-51, 1931.

Smith, Preserved. A history of modern culture. Volume I : The great renewal, 1543-1687. XI+672 p. New York, HOLT, 1930. ISIS

I owe to LYNN THORNDIKE's kindness a copy of this historical manual, the plan of which is very original. The limits chosen are already significant : 1543-1687, that is, from COPERNICUS and VESALIUS to NEWTON. It is divided into four parts : 1. The sciences (160 p.); 2. The humanities (133 p.); 3. Social control (208 p.); 4. The spirit of the times (84 p.). The first part —more than one quarter—is a history of science. Let us hope that the author's initiative will receive all the encouragement it deserves. G. S.

46. — HISTORY OF LANGUAGE, WRITING AND LITERATURE

Degering, Hermann. Die Schrift. Atlas der Schriftformen des

Abendlandes vom Altertum bis zum Ausgang des 18. Jahrhunderts. 38 p., 240 pl. (*Wasmuths Werkkunst-Bücherei*, Bd. 6). Berlin, WASMUTH, 1929. ISIS

Reviewed by H. JENSEN, *OLZ*, 33, 863-65, 1930.

Mawer, Allen. Problems of place-name study. Being a course of three lectures delivered at King's College under the auspices of the University of London, XI+140 p. Cambridge Univ. Press, 1929. ISIS

Reviewed by KARL LUICK, *Deutsche Literaturzeitung*, 1, 1898-1899, 1930.

Mladenov, Stefan. Geschichte der bulgarischen Sprache. XIV+354 p., 1 map. (*Grundriss d. slav. Philol. u. Kulturgesch.* Hrsg. v. REINH. TRAUTMANN u. MAX VASMER. Bd. VI). Berlin, DE GRUYTER. 1929. ISIS

Reviewed by E. TANGL, *DLZ*, 1, 2431-33, 1930.

48. — HISTORY OF PHILOSOPHY

(See also above, 18. Philosophy of science)

Ohmann, O. JOSEPH PETZOLDT zum Gedächtnis. *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik*, 13, 199-214, 1930. ISIS

Russell, Bertrand. Mensch und Welt. Grundriss der Philosophie. 343 p. München, Drei Masken Verl., 1930. ISIS

Reviewed by WILHELM BURKAMP, *Deutsche Literaturzeitung*, 1, 2070-72, 1930.

49. HISTORY OF RELIGION

(Science and religion)

Doninck, A. van. Ste-Dimphna-cultus en gezinsverpleging. *Bijdragen tot de geschiedenis der geneeskunde*, 10, 307-14, 1 pl., 1930. ISIS

Dunham, Chester Forrester. Christianity in a world of science. New York, MACMILLAN, 1930. ISIS

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Reviewed by FRIEDRICH HEILER, *Deutsche Literaturzeitung*, 1, 2358-59, 1930.

Rüsche, Franz. Blut, Leben und Seele. Ihr Verhältnis nach Auffassung der griechischen und hellenistischen Antike, der Bibel und der alten alexandrinischen Theologen. Eine Vorarbeit zur Religionsgeschichte des Opfers. 471 p. (*Stud. z. Gesch. u. Kult. d. Altertums, i. A. d. Görres-Ges.* hrsg. v. E. DRERUP, H. GRIMME, J. P. KIRSCH. 5. Ergänzungsbd.) Paderborn, SCHÖNINGH, 1930. ISIS

Reviewed by FRIEDRICH PFISTER, *Deutsche Literaturzeitung*, 1, 2402-05, 1930.

VII. MEDICINE

50. — HISTORY, ORGANIZATION, AND PHILOSOPHY OF MEDICINE

Beaudouin, Frédéric. Nécrologie par PAUL DELAUNAY. Le docteur FRÉDERIC BEAUDOUIN, d'Alençon (1856-1930). *Bulletin de la société française d'histoire de la médecine*, 24, 275-77, 1930. ISIS

Bellin du Coteau, Marc ; Bergeron, Marcel. L'histoire de l'éducation physique dans ses rapports avec la médecine. *Bulletin de la société française d'histoire de la médecine*, 24, 215-28, 1930. ISIS

Bernhard, Oskar. Die historische Entwicklung der Lichttherapie. (*Handbuch der Lichttherapie*, hrsg. von W. HAUSMANN und R. VOLK. S. 3-12). Wien, SPRINGER, 1927. ISIS

Burr, C. B. Medical history of Michigan. Vol. 1, compiled and edited by a committee, C. B. BURR, chairman. Published under the auspices of the Michigan State Medical Society. Minneapolis, BRUCE Publ. Co., 1930. ISIS

Reviewed by FRANCIS R. PACKARD, *Annals of Medical History*, 2, 714-15, 1930.

Bishop, W. J. English physicians in Russia in the sixteenth and seventeenth centuries. *Proc. Roy. Soc. Med.*, 23, 143-52 (Sect. History Med. 15-24) ; 1929. ISIS

Cabanès, Augustin (d. 1928). Le costume du médecin. Edité par les laboratoires P. LONGUET, 95 p. Paris, 1927. ISIS

Comrie, John Dixon. History of Scottish medicine to 1860. 301 p. Published for the Wellcome Historical Medical Museum by BAILLIERE, TINDALL & COX, London, 1927. (*Research Studies in Medical History*, no. 4). ISIS

Ebstein, Erich. Die Entwicklung der klinischen Thermometrie. *Ergebnisse der inneren Medizin und Kinderheilkunde*, 33, 407-503, 1928. ISIS

Ebstein, Erich. Historical jottings in prophylaxis. *Medical Life*, 710-12, 1930. ISIS

Fischer, Alfons. Beiträge zur Kulturhygiene des 18. und zu Beginn des 19. Jahrhunderts im Deutschen Reiche. VI+116 p. (*Studien zur Geschichte der Medizin*. Hrsg. von KARL SUDHOFF und HENRY E. SIGERIST). Leipzig, 1928. ISIS

Garrison, Fielding Hudson. Available sources and future prospects of medical biography. *Bull. New York Acad. Med.*, 4, 586-607, 1928. ISIS

Greve, H. Christian. Aphorismen zur Kulturgeschichte der Zahnheilkunde und des zahnärztlichen Standes. 91 p., 22 fig. Leipzig, THIEME, 1930. ISIS

Griesbach, Hermann. Medizinisches Wörter- und Nachschlagebuch. Ein Hilfsbuch für Studierende und Ärzte und alle mit der Medizin im Zusammenhang stehende Berufe. Mit Ableitung, Übersetzung und Erklärung der in der Medizin vorkommenden Fachausdrücke und mit biographischen und literarischen Angaben. 815+313 p. Giessen, ALFRED TÖPELMANN, 1927. ISIS

Haberling, Wilhelm. Festgabe zum sechzigsten Geburtstag, bearbeitet und herausgegeben von PAUL DIERGART. *Proteus*, Band 1, p. 5-45, Bonn, 1931. ISIS

The papers included in this Festschrift are classified in the present bibliography each under its own section. G. S.

Kruif, Paul de. Microbe hunters. 320 p. (*The life and letters series.*) London, CAPE, 1930. ISIS

Nicaise, Victor (1871-1930). Nécrologie par PAUL DELAUNAY. *Bulletin de la société française d'histoire de la médecine*, 24, 278-79, 1930. ISIS

Nuyens, B. W. Th. Bibliotheca medica Neerlandica. Edited by B. W. TH. NUYENS. With introduction in English and plates. Amsterdam, Internationaal Antiquariaat, 1930. ISIS

Opuscula selecta Neerlandicorum de arte medica. Fasciculus

octavus quem curatores miscellaneorum quae vocantur Nederlandsch Tijdschrift voor Geneeskunde collegerunt et ediderunt. LXVIII+ 314 p. Amstelodami sumptibus societatis, 1930. ISIS

Vol. 7 of this valuable collection containing the Surgery of MASTER THOMAS SCELLINGK was published in 1928 and reviewed in *Isis*, 12, 152-53. Vol. 8 contains three treatises by ANDREAS VESALIUS (1539), JODOCUS LOMMIUS (1560) and PETRUS FORESTUS (1589). They are included in the present bibliography under XVI(1)D and XVI(2)D. G. S.

Pagel, Walter. HELMONT. LEIBNIZ. STAHL. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 19-59, 1931. ISIS

Parry, Leonard Arthur. Some famous medical trials. X+326 p. London, J. and A. CHURCHILL, 1928. ISIS

Parsons, Robert P. History of Haitian medicine. With foreword by EDWARD R. STITT. 196 p., 21 fig., 1 map. New York, HOEBER, 1930. ISIS

Reviewed by THOMAS MCCRAE, *Annals of Medical History*, 3, 118, 1931.

Peachey, George C. The gold-headed cane. (By WILLIAM MACMICHAEL). A new edition with an introduction and annotations. XIX+195 p., 6 pl., illus. New York, MACMILLAN, 1930. ISIS

First published anonymously in 1827, the perennial enthusiasm for this historical classic is attested by subsequent editions in 1828, 1884, 1915 (reprinted in 1920), and in 1923 (reprinted in 1930). The cane, in delightful autobiographical style, chats about the great medical events of England, and relates most entertainingly the lives of its successive masters, JOHN RADCLIFFE (1650-1729), RICHARD MEAD (1673-1754), ANTHONY ASKEW (1722-1774), WILLIAM PITCAIRN (1711-1791), DAVID PITCAIRN (1749-1809), and MATTHEW BAILLIE (1761-1823). This edition is embellished with a valuable historical introduction and annotations, and is well indexed.

C. D. L.

Planer, Reinhard. Der Kampf um die Homöopathie *pro et contra*. 354 p. Leipzig, HÜGEL, 1926. ISIS

Plarr, Victor. PLARR'S lives of the Fellows of the Royal College of Surgeons of England. (THELWALL THOMAS Memorial). Revised by Sir D'ARCY POWER, with the assistance of W. G. SPENCER and G. E. GASK. Vol. 1, XXVI+752 p. Vol. 2, 596 p. Bristol, WRIGHT, 1930. ISIS

Reviewed by W. R. L., *Nature*, 126, 513-14, 1930; by FRANCIS R. PACKARD, *Annals of Medical History*, 3, 120-21, 1931.

Robinson, Victor. Specimen questions and answers, from the lectures

of VICTOR ROBINSON. Second edition. 32 p. Temple University School of Medicine, Department of History of Medicine, 1930. ISIS

Rolleston, Sir **Humphry.** Aspects of age, life and disease. 304 p. (*The Anglo-French Library of Medical and Biological Science*, ed. by F. G. CROOKSHANK and RENÉ CRUCHET). ISIS

Rolleston, Sir **Humphry.** Internal medicine. IX+92 p. (Volume 4 in the *Clio Medica Series*). New York, HOEBER, 1930. ISIS

Rather than a critical discussion of the changing theories and principles of internal medicine as reflected in notions of etiology, or in practical diagnosis, treatment, or prognosis, this review is more concerned with an account of discoveries in the fundamental medical sciences as related to internal medicine. There is a long index of personal names, and a separate subject index. It is unfortunate that such little space was permitted the author to develop his theme. The volume might invite greater interest and stimulate to further reading were it provided with a few illustrations and a workable bibliography.
C. D. L.

Schenck, H. P. The Mutter Museum of the College of Physicians of Philadelphia. *Annals of Medical History*, 3, 69-74, 4 fig., 1931. ISIS

Sigerist, Henry E. Probleme der medizinischen Historiographie. *Sudhoffs Archiv für Geschichte der Medizin*, 24, 1-18, 1931. ISIS

Sperling, Arthur. Die Brücke zur Homöopathie und der Weg zur Einheit der Arzneimittellehre. Hist., biolog., therapeutische Studien für Arzt, Studierende u. alle Gebildeten. X+256 p. Leipzig, W. SCHWABE, 1927. ISIS

Sudhoff, Karl. Aus Forschung und Lehre der Medizingeschichte. Bibliographie der medizingeschichtlichen Arbeiten KARL SUDHOFFS aus den Jahren 1923-1928. Nach Materien geordnet von GRETE HERBRAND. *Kyklos*, 2, 275-86, 1929. ISIS

Temkin, Owsei. Beiträge zur archaischen Medizin. *Kyklos*, 3, 90-135, 1930. ISIS

Temkin, Owsei. Studien zum « Sinn »-Begriff in der Medizin. *Kyklos*, 2, 21-105, 1929. ISIS

1. Kritisch-logische Voruntersuchung; 2. Krankengeschichte und Sinnsphäre der Medizin; 3. MOLIÈRE und der Sinn der Medizin im 17. Jahrhundert; 4. Epidemiologie und Geschichte der Medizin.

Thompson, Charles John Samuel. Guide to the surgical instruments and objects in the historical series, with their history and development. With a foreword by the conservator, Sir ARTHUR KEITH. 92 p. (Museum, Royal College of Surgeons of England). London, TAYLOR and FRANCIS, 1930. ISIS

Thompson, Charles John Samuel. The quacks of old London. London, BRENTANO'S, 1928. ISIS

Tobey, James Alner. Riders of the plagues : the story of the conquest of disease. XV+348 p., 8 pl. New York, SCRIBNER'S, 1930. ISIS

Viets, Henry R. A brief history of medicine in Massachusetts. Boston, HOUGHTON MIFFLIN, 1930. ISIS

Warthin, Aldred Scott. The physician of the dance of death. *Annals of Medical History*, 2, 697-710, 7 fig., 1930.; 3, 75-109, 28 fig., 1931. ISIS

Weinberger, Bernhard Wolf. Dental literature ; its relation to development of better dentistry. *Dental Survey*, 5 p., September, 1930. ISIS

Welch, William Henry. The WILLIAM HENRY WELCH jubilee, Washington, April 8, 1930. *Isis*, 14, 423-24, 1930. ISIS

51. — EPIDEMIOLOGY, HISTORY OF SPECIAL DISEASES, PUBLIC HEALTH, AND SOCIAL MEDICINE

Bókay, Joh. v. Unsere Kenntnisse über den Krupp von FR. HOME (1765) bis zum Ausschreiben der internationalen Napoleonischen Preisfrage (1807-1809) und die Ergebnisse der letzteren. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 79-89, 1931. ISIS

Fraser-Harris, D. F. Smallpox in non-medical literature. *Medical Life*, 37, 552-67, 1930. ISIS

Gill, Clifford Allchin. The genesis of epidemics and the natural history of disease. London, BAILLIÈRE, TINDALL, and COX, 1928. ISIS

Kannabich, J. W. Istoria psychiatrii (Geschichte der Psychiatrie). 520 p. Moskau Medizin. Staatsverlag, 1929. ISIS

Reviewed by H. ZEISS, *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*, 29, 189-90, 1930.

La tubercolosi. Scienza e legge nella lotta contro la tubercolosi traverso i tempi e nei diversi paesi. Ministero dell'Interno. Direzione Generale della Sanità pubblica. Vol. 1. XLIV+720 p. Roma, Provveditorato generale dello Stato, 1928. ISIS

Meyer, Willy. Cancer, its origin, its development and its self-perpetuation. The therapy of operable and inoperable cancer in the light of a systemic conception of malignancy. XXXIII+427 p., ports., New York, HOEBER, 1931. ($ 7.50). ISIS

« The book deals with the tumor cancer as an ordinary, lawful reaction of the tissue to certain systemic and local morbid conditions of the human organism. The book denies the autonomy of cancer. It asserts the probable unity in character of all types of new growths. It recognizes only one ailment, chronic irritation, as the potential source of all of them. It tries to make the view acceptable that the chronic irritation effective in cancer is a composite of a systemic chronic irritation and a local chronic irritation, that both of them are singly harmless, but potentially harmful when acting jointly. It finds a multiplicity of factors, among themselves unrelated and from one another widely differing, capable of giving rise to the local chronic irritation. For the development from chronic irritation of a precancerous state and the progression of the latter to cancer, the book holds predisposition to cancer, inherited or acquired, as indispensible. It calls probably predisposed to cancer all persons whose body fluids react with a higher than the normal degree of alkalinity, probably immune those troubled with acidosis. » The book contains abundant bibliographical references. No attempt is made to tell the history of cancer investigations, but there are a series of portraits of the leading deceased investigators : RUDOLF VIRCHOW, JOHANNES FIBIGER, KATSUSABURO YAMAGIWA, JOHANNES P. MÜLLER, ROSWELL PARK, HARVEY RUSSELL GAYLORD, ERWIN F. SMITH, JACQUES LOEB, MARIN THÉODORE TUFFIER, ERNEST FRANCIS BASHFORD, BERTIE RONALD GORDON RUSSELL.

G. S.

Oliver, Wade W. Stalkers of pestilence. The story of men's ideas of infection. Introduction by THEOBALD SMITH. 251 p., 23 fig. New York, HOEBER, 1930. ISIS

Reviewed by THOMAS MCCRAE, *Annals of Medical History*, 3, 121-22, 1931.

Robinson, Victor. Historical aspects of physiotherapy. *Medical Life*, 37, 533-51, 11 ills., 1930. ISIS

Senk, Herbert. Ein kartographischer Beitrag zur Geschichte des Englischen Schweisses. *Kyklos*, 3, 273-91, 8 maps, 1930. ISIS

« ' Sudor anglicus ', nach den älteren Autoren auch ' Pestis britannica ', ' Ephemera britannica ' oder nach den Chroniken ' Swetynge sykenesse ', sweating sickness, bezeichnet eine erstmalig zu Ausgang des 15. Jahrhunderts auftretende Krankheitsform, die durch ihre eigenartigen Symptome, durch die Art ihrer Verbreitung und ihre hohe Mortalitätsziffer in der Geschichte

der Seuchen eine besondere Stellung einnimmt. Die Erkrankung hat man nach einem ihrer Hauptsymptome und dem Land, in welchem sie zuerst und am meisten aufgetreten ist, Englischer Schweiss benannt. Ein Überblick über die gesamte Literatur lehrt uns mit Sicherheit, dass die Krankheit innerhalb eines Zeitraumes von 66 Jahren fünfmal in England epidemisch auftrat und nur einmal, im Jahre 1529, den Boden ihrer Ursprungsstätte überschritt, um einen Teil Mitteleuropas in Form einer wahren Pandemie heimzusuchen. Sie ist alsdann nach einem Zeitraum von 22 Jahren im Jahre 1551 letztmalig als Epidemie in England aufgetaucht und beschränkte sich wieder auf dieses Land. Wieweit anderen Krankheitsbildern aus früheren oder späteren Jahrhunderten eine Identität mit dem Englischen Schweiss zugeschrieben werden darf, möchte ich an dieser Stelle keiner näheren Prüfung unterziehen. »

Storch, T. C. von. An essay on the history of epilepsy. *Annals of Medical History*, 2, 514-50, 1930. ISIS

Venzmer, Gerhard. Eine sterbende Krankheit. Vom Aufstieg und Niedergang der Syphilis. Dritte Auflage. 100 p., 54 fig. Leipzig, KONEGEN, 1929. ISIS

Vorwahl, H. Die Wurzeln der Sozialhygiene. *Sudhoffs Archiv für Geschichte der Medizin*, 23, 384-87, 1930. ISIS

52. — HISTORY OF HOSPITALS AND OF MEDICAL TEACHING

Bologa, Valeriu L. Die historischen Grundlagen des Medizinunterrichtes im heutigen Rumänien. *Sudhoffs Archiv f. Geschichte der Medizin*, 24, 90-116, 1931. ISIS

Croskey, John Welsh. History of Blockley ; a history of the Philadelphia General Hospital from its inception, 1731-1928. Illustrated with 15 pl. Philadelphia, DAVIS, 1929. ISIS

Reviewed by FRANCIS R. PACKARD, *Annals of Medical History*, 11, 245-47, 1930.

Cutter, J. B. Intern service in the old days at Chambers Street Hospital, N. Y. *Annals of Medical History*, 3, 39-49, 1931. ISIS

Holcomb, Richmond Cranston. A century with the Norfolk naval hospital, 1830-1930. A story of the oldest naval hospital, the medical department of the navy and the progress of medicine through the past one hundred years. Portsmouth, Va., Printcraft Publ. Co., 1930.

Reviewed by ROBERT P. PARSONS, *Annals of Medical History*, 3, 123-24, 1931.

West, Eva. The WILLIAM BEBB collection of prints of Paris hospitals. *California and Western Medicine*, 33, 9 p., 5 fig., 1930. ISIS

53. — PHARMACY, PHARMACOLOGY, TOXICOLOGY

Aronson, Samuel. Geschichte der Lachgasnarkose. *Kyklos*, 3, 183-257, 1930. ISIS

Couch, James Fitton. The toxic constitutent of richweed or white snakeroot *(Eupatorium urticaefolium)*. *Journal of Agricultural Research*, 35, 545-76, 1 fig., 1927. ISIS

« Among the many perils and hardships that marked the settlement of the old frontier of the United States was the mysterious malady known as ' milk-sickness. ' Appearing customarily late in the summer it rapidly assumed epidemic proportions, sparing neither age nor sex, frequently sweeping away a quarter of the population, and sometimes depopulating whole villages. Nearly every district in the old West—now the Central West—suffered at one time or another from this disease and so sudden was its onset, so recondite its cause, so unavailing were the preventive measures taken by the terrified backwoodsmen, and so fatal were the attacks, that the old chroniclers refer to it in the strongest terms as formidable, terrible, and frightful. NICOLAY and HAY describe a characteristic outbreak during which the mother of ABRAHAM LINCOLN died. » « At the present time the only hypothesis that has any standing is that richweed poisoning causes this disease in the East. All the experimental work reported in the last decade supports this point of view and demonstrates that Eupatorium urticaefolium is a distinctly poisonous plant and that milk obtained from cows that have fed on it is capable of producing the disease in other animals. »

Denninger, Henri Stearns. A history of substances known as aphrodisiacs. *Annals of Medical History*, 2, 383-93, 1930. ISIS

Heyser, Kurt. Die Alliumarten als Arzneimittel im Gebrauch der abendländischen Medizin. *Kyklos*, 1, 64-102, 1929. ISIS

Klüver, Heinrich. Mescal. The « divine » plant and its psychological effects. With an introduction by MACDONALD CRITCHLEY. 111 p. London, KEGAN PAUL, 1928. ISIS

Scheer, Hermann. Fragmente zur Arzneigeschichte. *Speculum*, 131-44, 1929. ISIS

1. Warum treiben wir heute Geschichte der Heilmittel?; 2. Einleitung zur Geschichte einer Heilpflanze; 3. Mensch und Pflanze.

VIII. EDUCATION

(The methods of accumulating, imparting and diffusing knowledge.)

54. — EDUCATION

(Generalities, methods, colleges, universities)

Abel, John J. The education of the superior student. *Journal of Chemical Education*, 7, 283-93, 1930. ISIS

Flexner, Abraham. Universities : American, English, German. x+ 381 p. New York, Oxford University Press, 1930. ISIS

56. — BIBLIOGRAPHY

(Methods, libraries)

Pierpont Morgan Library. A review of the growth, development and activities of the library during the period between its establishment as an educational institution in February 1924 and the close of the year 1929. 14 pl. 148 p. New York, 1930. ISIS

Admirable review of the progress made by the Library during the last six years. Among the new acquisitions, the most interesting to our readers are : The holograph MS. of JOHN LOCKE's *An essay concerning humane understanding* (Books I-II, 383 p.) dated 1685 (p. 29). MSS. of the *Erotemata* of MANUEL MOSCHOPULOS ; of the *Thesauro di cavalli* of BONIFACE OF CALABRIA, translated by ANTONIO DI PERA (with 158 pen-and-ink drawings) ; of HAYTON, *La fleur des histoires d'Orient ;* of MARCO POLO (XIVth century French text, CEPOY version). Letters and documents of FRANCIS BACON, SAMUEL DE CHAMPLAIN, GALILEO, etc. The Pierpont Morgan Library does not simply contain the richest collection of illuminated MSS. in the New World and many other treasures ; it has become one of the leading centers of research, and under the direction of its great patron, Mr. JOHN PIERPONT MORGAN (the founder's son) and of its eminent director, Miss BELLA DA COSTA GREENE every effort is made to increase its scientific value. May it prosper !

G. S.

57. — MUSEOLOGY

(Museums and collections)

Courret, G. Le « Musée de l'avenir » de GEORGE SARTON. *Annales de la société scientifique et littéraire de Cannes*, 2, 75-79, 1930. ISIS

Apropos of The Museum of to-morrow published in *Natural History*, 1924 (*Isis*, 7, 282).

IX. ALIA

59. — ERRATA. Series 22.

(For previous errata, see Isis, 15 319*)*

Vol. 11, 220. note CRUM, CHARLES GEORGE, not Crumpt.

Vol. 11, 423. note SOOTHILL. *Geogr. Journal*, vol. 69, 3 pl.

Vol. 14, 19, l. 5 of P.S. read SACHS instead of Sach.

Vol. 15, 286. Note RÂGHIB, l. 6, read physiognomy instead of physionomy.

SARTON's *Introduction to the History of Science*, vol. I.

P. 310. Note on CHANG CHUNG-CHING, l. 12, read Shô-kan-ron instead of Sho-kan-ron. Same correction in index, p. 830.

P. 640, l. 11. For istiqsāt read iṣtiqsāt or better iṣtaqisat.

P. 727. Note ALPHANUS, l. 5. Read Premnon instead of Prennon.

Author's Index to the Thirty-first Bibliography

The Roman figures refer to centuries ; the Arabic figures, to the sections of Parts II and III, which are numbered consecutively from 1 to 59. This index will enable one to find more easily the papers analyzed in the present bibliography, and also to see at a glance what each writer is doing.

April 1931. FRANCES SIEGEL.

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